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LETTERS OF

BARROW, FLAMSTEED, WALLIS, AND NEWTON,

FROM THE ORIGINALS.

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CORRESPONDENCE

OF SCIENTIFIC MEN

OF THE SEVENTEENTH CENTURY,

INCLUDING LETTERS OF

BARROW, FLAMSTEED, WALLIS, AND NEWTON,

PRINTED FROM THE ORIGINALS

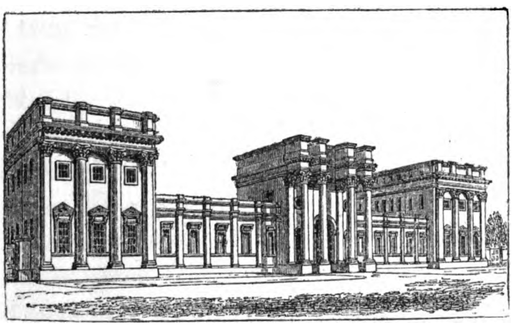
IN THE COLLECTION OF THE RIGHT HONOURABLE THE

EARL OF MACCLESFIELD.

BIBLIOTHECA·BODLEIANA·

IN TWO VOLUMES.

VOL. I.



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PREFACE.

THE following pages contain a selection of the letters of Scientific Men of the seventeenth century. They extend over a portion of time from 1606 to 1741; and when it is recollected that within that time Oughtred, Flamsteed, the Gregorys, Barrow, Wallis, and, lastly, Newton wrote, it may well be considered a century which was one of the most eventful in the scientific progress of this country. It is not intended here to enter into any detailed account of the letters themselves, or their contents; but there are other names which may be mentioned, examples of whose correspondence will be also found, Gascoigne, Briggs, the Pere Berthet, Huygens, and lastly, Collins, to whose indefatigable activity indeed, as a correspondent, it will be seen that we are indebted for much which is valuable, not merely from his own hand, but from that of others.

Perhaps there are few means by which so much light may be thrown on the stages through which men's minds advance in the progress of discovery, as by the written transcript of their occupations, their thoughts, and their difficulties, which is presented by the daily record of a private correspondence. And it was with this view that the work was originally recommended to the Delegates of the University Press. The autograph originals were supplied by the Earl of Macclesfield. They had passed into the possession of

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the first Lord from Jones, and although the exact nature of their contents had not been ascertained, yet it had long been known that many manuscripts of value were contained in his Lordship's collection; and when on inspection it was stated that the publication of the present series was desirable, the same liberality which laid open the library to inquiry at once assented to any use being made of such among its contents as from their nature might be likely to promote the history of science.

It will be known to most of those into whose hands these volumes will pass, that my father was engaged upon them at the time when he was suddenly taken from his labours. He had printed the first volume, and I therefore entered upon the work with the second, the first sheet of which had been once corrected, but not revised. The state of the materials which were left to me will be seen hereafter; but for the execution and correctness of the second volume I must be responsible.

There is reason to think that he contemplated the preface as likely to be a work of considerable extent, and I have little doubt that he intended to embody in it many particulars connected with the history either of the papers themselves, or of their authors. To any one who remembers his powers of research and accuracy of combination, the loss of such a preface will not appear light; but it is impossible for me to supply it. The notes which he left of his intended preface amount to less than a single page of disconnected writing, and these few memoranda relate almost exclusively to the system pursued in typography, the method of algebraical notation, or the meaning of peculiar marks. These are all the materials which were left me for this part of my office. It is true, that from other sources abundance of biographical matter might be drawn which might easily be made prefatory to the correspondence; but it has been thought better to avoid it. The notes, it is hoped, may supply all which is really necessary on this head; and to

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lengthen the introduction by matter of this kind, which is easily compiled from well known sources, or in more singular features though known to few, is also interesting but to few, might appear a mere affectation in another, though he would have known both what was valuable and what was interesting. With this brief notice I must pass to that which more immediately concerns my execution of the task intrusted to me.

In the first place it must be observed, that from the beginning the printers declared themselves unable to work from the originals. There were so many different handwritings, so many various forms of spelling, different abbreviations, and distinct methods of notation, that it was found impossible for any one unacquainted with the general subjects of the letters to decipher, or reproduce them; and hence it became necessary to copy *the whole correspondence.* And this was done by my father. There were advantages which flowed from the labour, troublesome as it was. Had the letters been at once printed from the originals, they must have been made copies of them in all the irregularity and quaintness of the spelling of the time. It would have been vain to attempt to systematize, as one part followed another through the press; and yet, curious as the letters might have been in this form, it may well be doubted if any advantage could have resulted from adhering to inaccuracies, while every convenience is gained by the simple introduction of one regular orthography. In this single respect they must not be taken as specimens of antiquity. The spelling has been modernized throughout[a]. And if any additional reason were wanting, it would suffice to say, that it sometimes varies almost as much in the same, as it does in different individuals. This

[a] It will be seen that the principle has been extended to the French as well as the English letters; and in all cases these have been submitted to the revision of a French gentleman resident in the university. On one or two occasions a few words of Dutch occur; and though in some particulars it was possible to get rid of the antiquated form of the words, yet there are others which still remain questionable, and which, I am afraid, a modern writer would hardly admit.

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is most remarkable in the case of proper names[b]. In no case, however, can these alterations produce confusion; and though it may be thought that it would have been better implicitly to follow the MS., yet the changes were not made without consideration, and it appeared that thus the real object of the work was more likely to be secured, namely, to throw light on the scientific progress of a remarkable period, rather than to produce facsimiles of the correspondence of individuals.

Again, it was determined from the first that all letters should be noticed. Many may appear very brief; some hardly worthy of preservation; others have been printed before: but on the whole the plan which has been followed appeared the most satisfactory. In this case, the excess were a preferable fault to the defect. Imperfect extracts are rarely held sufficient; and when once letters are omitted, any reader may imagine that the one which would have possessed the most peculiar interest to himself is the one which has been neglected. No letter, therefore, and no passage has been rejected without a notice in the page of its contents, and a reference to the place where it may be found, or a foot note giving a particular reason for its non-appearance in the text.

One evil which was found to pervade the letters of many of the writers to a greater or less degree, was the frequent introduction of interlineations, or insertions of considerable extent. These are most frequently in the hand of the author

[b] There are some few differences in this respect between the first and second volume. Thus the name of Huygens is always printed Hugens in vol. i. There is authority for each in the MSS. as in the printed works of the same date. Newton himself makes it Huygens on more than one occasion. Townley's name is spelt by Flamsteed in every way which would represent the same word; and on the authority of the Phil. Trans., vol. vii. p. 5130, the present form has been adopted in preference to Towneley. Borellus and Borellius are used indiscriminately. Newton writes Gascoin, while in vol. i. the writer styles himself Gascoines. Bertet or Berthet has been adopted for the name of the Jesuit father in preference to Bertit, from the Biographie Universelle, as well as MS. authority. In James Gregory's correspondence, Sir R. Moray's name was inadvertently printed Murray. The other changes were advisedly made.

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himself, but they are sometimes in that of Collins, from his situation the general correspondent of a large portion of the scientific men of his day. Whenever the corrections of Collins have been adopted, the fact has been mentioned; but when the corrections were obviously those of the author of the letter, a different method has been adopted, according to the nature of the case. If the portion introduced ran on with the original, or wanted but a trifling change (as for instance that of punctuation) to make it do so, it has been employed without comment as a part of the text. But if it were discrepant, either the necessary alteration in the sense has been made, and mentioned, or the whole of the supplementary matter has been added in a note.

After what has been said, it will be anticipated that each several sheet, as it went through the press, has been corrected from the originals. Without this, no accuracy could have been expected; yet few will give credit to the care required; none perhaps could do so, unless they had seen how frequently at the first view the meaning or arrangement might be mistaken, of words, of passages, or even of mathematical operations. It was sometimes impossible that they should be understood until a long examination determined their nature, or their position in the discussion. Until this was done, indeed, they could have no meaning; and it was necessary first to discover the intention of the writer, and then reduce his apparent confusion to the order which had thus become evident. And in this way also the whole punctuation has been determined. To adhere to that of the original was impossible. Sometimes it was arbitrary, and the points were used in a manner very different to the force which we attribute to them; sometimes there were none: and therefore, throughout the work, the pointing has been determined according to the view taken of the sense. Long involved sentences have been divided into others, shorter and more simple. Parentheses have been introduced where they were not in the original, but appeared to be useful, however

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inelegant to an eye accustomed to modern typography; and they have yet more frequently been removed, when superfluous or redundant; though even now they are but too common, and might often, perhaps with advantage, have been thrown aside.

But there was a far more serious difficulty to be met than the arrangement of words or mathematical operations;—to correct the disordered chronology of the correspondence. It has been said that the letters were copied before they passed into my hands; but they were copied merely as they stood in the MS. Frequently, even according to their superscribed dates, they were wrongly placed in the collection, and these had been generally, though not universally, numbered so as to fall into their proper places. But some were without dates at all; others in entirely wrong positions, from the confusion of the old and new styles; and in the early part of any given year it was necessary to be very cautious. As for instance, it might sometimes be a matter only to be determined by comparison with others, or by the incidental notice of some fact in its contents, whether a particular letter, dated Jan. 1761, were of the year 60–1, or 61–2. There is but one letter whose date, to the end of the series of those of its author, it was found impossible clearly to make out, and that is the one which stands last in the collection of Newton's correspondence. Not unfrequently the address also was wanting: in some instances this could be determined, in others conjectured; occasionally, however, there were no immediate means from which it could be supplied.

The only great distinction which could be made between one part and another in classifying the letters themselves, is that which will be seen to exist as the difference between the first and second volumes. The former contains a more miscellaneous collection, the latter consists of certain portions of the correspondence of particular individuals, complete in themselves. They will frequently be found to bear upon each other, but the simplest arrangement was that which has

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been adopted; and it was also that in which the originals had been placed in Lord Macclesfield's portfolios. For instance, there are letters of Wallis in the first volume, but his correspondence with Collins, which occurs in the second, is a perfectly distinct series, and its perspicuity as such is better preserved than it might have been, had other letters been mingled with it. The several series in vol. ii. have been arranged according to the alphabetic order of the authors' names.

In some few instances the original papers do not appear in the collection, but a copy in a different hand, and apparently taken at the time, has been preserved. In several places this is in the hand of Collins. Occasionally, the original and also a copy have been found together; and these have been carefully collated, and the readings corrected from the comparison. The letters of Collins himself constantly bear evident marks of not having been the same which he sent. Many are unsigned, not folded as for the post, and without a regular address, though bearing the superscription "Mr. John Collins to &c. &c." They might be either first draughts of letters to be more formally put together afterwards, or copies, which he was perhaps in the habit of preserving: but often mere notes appear of what it was his intention to write, and even where there is more than this, they yet bear strong internal evidence of belonging to the former class, for they abound in alterations, or corrections, and additions, sometimes merely interlined, but often amounting to whole paragraphs, added in the margin, the lower part of the page, or on the opposite one. Nor do these always harmonize very perfectly with the sense at first designed. Throughout the work, however, no important alteration has been made in the text, even to correct what was clearly wrong, without its being mentioned. When it has been impossible to decipher words, a space has been left. The same has been done when words had been either obliterated by the wearing of the paper, or actually torn away by accident to the margin.

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But wherever it was possible to guess at the lost words, or parts of those which had suffered mutilation, they have been introduced between brackets.

Italic types have been employed for all algebraic expressions, Roman for reference to the diagrams, according to the system, and with the trifling varieties, to which the eye is now accustomed. There was no regular rule of this kind to be discovered in the originals; indeed, the change of notation in algebraic symbols had hardly yet been firmly established; as Collins testifies in writing to Wallis of the opposition made to that which we should have thought a natural as well as profitable suggestion[c]. Thus A^2 has been substituted for Aq wherever the latter might appear, for the MS. was not uniform in this respect. The form aa occurs as well as a^2. It would perhaps have been better to avoid this, but it was found to have been introduced into the printed pages of vol. i.; and after this, little was thought of a circumstance which could produce no confusion, and was to a certain degree characteristic of the age in which the writers performed their service to science.

On examining the preparations made for the second volume by my father, it was soon found that the notes were incomplete. In some cases they were inserted; yet even then, more than once, required alteration or correction: in others, materials were collected, but not arranged or reduced to form: and in many, a pencil mark in the margin pointed out that he had thought something required notice or illustration as he proceeded in his work of copying; but there was nothing to indicate what he had intended to introduce. After the correspondence of Flamsteed, it will be seen that many references are still made to the places in which letters had before been printed, but to call attention to these no mark had been made, or space left in the copy; although sometimes, but not in all cases, the reference had been inserted in

[c] Wallis's Correspondence; see vol. ii. p. 480.

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his catalogues of the MSS. Bayle's General Dictionary was always quoted in those catalogues, Birch's History of the Royal Society not so universally, and the references to the Phil. Trans. are almost entirely my own. No one, however, except myself, would have known how he had generally determined to proceed. For all notes of importance his materials were at hand, the arrangement of which in some parts few could so well have understood: and though far from completing that to which he had looked forward, yet it is hoped that they will be found to embody some part at least of that which had been expected when he undertook to edit these papers. The notes to Flamsteed's correspondence were still unfinished[d]. After these, the larger portion were compiled and added by a weaker hand. In almost every instance his references have been verified; and here, as ever, his singular accuracy was apparent.

The notes are generally of three kinds: they are biographical, or bibliographical; they relate to the state or meaning of the text; or, thirdly, they are notes of reference. They have been made as few and as short as possible. A slight knowledge of catalogues is sufficient to enable any one to multiply bibliographical notices to almost any extent. But this would have been a needless augmentation of the volumes, nor is it easy to determine what in this department may be unknown, or really curious. I have been therefore guided partly by the marginal pencil marks which have been mentioned, partly by what appeared necessary for illustration, or threw light upon the date of any letter or the facts contained in it. The biographical notes are always derived from collections made for the purpose, before my task began; and according to the intention with which the publication was commenced, nothing has been mentioned of any individual,

[d] For example; the notice of Horrox's Opera Posthuma was suggested by a paper lying between the leaves of the original paper where it occurs, and was taken partly from this, and partly from MS. memoranda in his own copies of the book.

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however interesting, the facts of whose life are already well known. It has been said that no important correction of a doubtful passage in the text has been made without stating the change, or the reasons which made it requisite. In like manner, incorrect mathematical operations have been restored; but there were some cases, in which the full investigation of a problem would have rendered the foot notes of equal length with the matter to which they belong, and therefore a caution has been once or twice given, where other and obvious mistakes laid the accuracy open to suspicion. But there were other errors of frequent or almost constant occurrence, from the hurry of letter writing, the omission of a coefficient or an exponent, the substitution of a wrong sign, or an error on the same points, when there was no omission, and the next line, or some subsequent statement, might correct it, so that even the mistake did not lead to an ultimately false conclusion. To leave such errors, from a notion of fidelity, would have been absurd, and to make a note of every such emendation would have been equally useless. Of one only class of the remaining notes, those of reference, is it necessary to speak. It has been said, that many of the collection have been printed before, and, as a general rule, the place in which they will be found is given. An exception will be observed in the case of two or three which had been employed in the Commercium Epistolicum, and also those few which will be found in the fourth volume of Horsley's Newton. It was at one time my intention to have indicated these by attaching an asterisk to them in the index. But it has been determined that no index should be given. Each entry would indeed only consist of the number of the particular letter in the collection, the date, the name of the author, and that of the person to whom it was addressed. And this would have been a barren labour, though one which would have consumed some space and expenditure. Nor does it appear from any entries of my father's that he had intended particularly to notice these

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letters, probably as being too well known to make it necessary. From No. CCXXXV. to No. CCLXXV. twelve will be easily recognised as following Newton's Optics in Horsley's edition of his works. And those to whom the Commercium Epistolicum is familiar will at once supply the omission for the extracts which are included in No. CCI. or for No. CCL.

There is, perhaps, but one subject more, which must be mentioned. Two years may seem a long time to have been occupied in the production of a volume of six hundred pages; but let it be remembered that it was undertaken, and has been carried on, under the pressure of other occupations; that it has been the work of what would else have been the hours of leisure from duties in themselves laborious, and pursued under the depression of health previously shaken, and hardly, until lately, certain and unfailing. With this brief notice it must pass before the world.

STEPHEN JORDAN RIGAUD.

Exeter College,
June 14th, 1841.

The following letters are printed in Horsley's Newton, vol. iv.

CCXXXV. CCXXXVI. CCXXXVII. CCXXXIX. CCXLIII.
CCL. CCLII. CCLIII. CCLXIII. CCLXV. CCLXVI.
CCLXXV.

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LETTERS

OF

SCIENTIFIC MEN.

I.

H. BRIGGS TO MR. RALPH CLARKE AT GRAVESEND.

Salutem in Christo,

GOOD Mr. Clarke, I heartily thank you for your great pains: I must still remain your debtor many ways. I do here at length send you your ruler. For the plain timber I hope you know how to use it. You have on the one margin the numbers of the inches of length of a foot, on the other side the numbers of the transverse lines, in the middest the numbers of the parallels. You must take the greater number in the parallels, the less in the transverse lines, as 19 and 13 is almost 7 in length, 13 and 9 give $14\frac{3}{4}$, 10 and 9 give $19\frac{1}{5}$, 9 and 8 give 24. Some of the transverse lines have three parts, as 10, 9, 8; others two parts, as 14, 13, 12, 11; the other, that are greater, have but one part. If your greater number given be under 11, see the concourse of the parallel and the lowest part of the transverse; if above 10, and less than 15, see the concourse in the middle part of the transverse; if 15 or greater, see the concourse in the highest parts of the transverse lines.

For your round timber you have two pricked lines, which do, as the transverse, go doubling from side to side; they serve to give the length of a foot when we

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have given either the whole thickness that is the diameter of the one end, or $\frac{1}{4}$ of the compass about, that line which in the parallel of 24 beginneth at $2\frac{7}{20}$ inches going aslope to $6\frac{1}{2}$ on the other side and so to $12\frac{4}{5}$, and lastly to $21\frac{1}{5}$ is for the quarter of the compass about, where you see the parallels produced till they reach unto this line; as, if the pillar be 96 inches about, $\frac{1}{4}$ is 24, and the length of a foot of such a pillar of timber or stone is 2 inches and $\frac{7}{20}$, as appeareth in the parallel of 24; but if $\frac{1}{4}$ be 15 inches, the length of the foot is $6\frac{3}{100}$; if $\frac{1}{4}$ be 14, it is $6\frac{92}{100}$; if 11, it is $11\frac{22}{100}$; if 10, it is $13\frac{57}{100}$; if 8, it is $21\frac{21}{100}$.

The other line beginneth at $3\frac{82}{100}$ in the parallel of 24 and goeth to $10\frac{4}{7}$, and so to $20\frac{2}{3}$: this serveth when we have a diameter of the end given; as, if it be 24 thick, the length of the foot is $3\frac{82}{100}$; if the thickness be 15, it is $9\frac{78}{100}$; if 14, it is $11\frac{45}{200}$; if 11, it is $18\frac{18}{100}$; if 10, it is 22 in length, &c.

If there be any thing wherein you are not fully satisfied, I pray you write a word or two. Let nobody have a copy of it I pray you. If any be desirous, I will find convenience to help him to one at a convenient rate. It may be I shall not go hence this week. Thus in haste, commending me very heartily to yourself and Mrs. Clarke, to Mr. Berdesworthe and Mrs. Berdesworthe, I take my leave, commending us all to the Lord's merciful protection. Farewell from G. H. this 25th of Feb. 1606.

Your assured friend to his power,

H. Briggs.

This is the letter mentioned by Ward in his Lives of the Professors of Gresham College (p. 129). G. H. probably stands for Gresham House, since the college had been the residence of its founder. The original contains two papers simi-

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lar to each other, and one of them is entitled, "for the delineation of Bedwell's ruler," which is evidently the instrument to which the letter alludes. These are not now printed, as reference may be made to Bedwell's translation of Lazarus Schoner, De Numeris Geometricis, (4°. Lond. 1614,) "teaching the fabric" of his ruler, or to Mesolabium Architectonicum, (4°. London, 1631,) in which an account of it is "declared by Wilhelm Bedwell his nephew, vicar of Tottenham."

II.

J. HALES TO REV. W. OUGHTRED AT ALBURY.

Good Mr. Oughtred,

Since your being with us at Eton, I was but one three days absent, and then only fell out the opportunity of answering your letters, eruditas, bone Deus! et perhumanas: which by being at Oxford I unfortunately missed. Now verily, sir, I must needs confess, that such kindness and so beneficial, upon so small acquaintance, I never received at the hands of any man, as I have at yours. Either your facility was great or your pains very much, who could in so short a space discharge yourself of so many queries. But howsoever, I esteem your courtesy above all the rest. Amongst all the solutions which you sent me, none there was which gave me not full and sufficient satisfaction, (and so I persuade myself would have given to one of deeper skill than myself,) one only excepted, and that is concerning the projecture of an oblique circle. I must confess I cannot well put by your demonstration, neither indeed, to speak plainly, do I throughly conceive of it, by reason, I doubt not, of my being unexperienced in these studies: for if I well conceive the nature of the projection, (which I

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take to be nothing else but the representations of some shape and figure in plano, accordingly as it appears to the eye [a],) I do not see how your conclusion can be good, except it be granted that there is no means to express an oblique circle according as it appears to the eye, which is against your own experience: for even in the universal astrolabe there is one only circle fully and circularly projected, and one projected into a straight line, all the rest are either ellipses or else figures drawing near unto the nature of ellipses, composed of two arches of circles, which I think indeed to be ellipses: for if so be every meridian in the universal planisphere be to be projected as a circle, then why are they not indeed all circles, since that the astrolabe is nothing else but the projection of all the rest of the meridians in the plane of one? If I take upon me to dispute with you, it is but only to learn, and learn I cannot of you, except I bewray my ignorance unto you; and assure yourself I will most shamelessly confess it unto you, that I may receive information from you. But I would not wish you to trouble yourself about this business, for I am now upon going to Oxford, not to return to stay ti[ll] about a twelve or fourteen days before Christmas, about which time I [under]stand by your father you are purposed, God permitting, to be here. For that . . . . . private matter about which you wrote, I must confess I have thought more [upon] it than ever I did in my life; but what the reasons are why I remain irresolute, I will throughly acquaint you when I can speak with you. You shall

[a] [Note written on the margin of the letter:] No; not as it appears to the eye: but as the visual rays coming from the eye by the circumference of a circle inscribed upon the globe do terminate upon a plane diametrically opposite to the eye: the eye also itself must be understood to be placed in the very end of the diameter.

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receive by your man your little compendium of triangles, by which I must confess I have found myself much eased. And now what is it that I can return you for all these exceeding courtesies? But I do not love to compliment; and if I mistake you not, you do not expect it. I pray let me be remembered, though unknown, to Mistress Oughtred: and so commending my love unto you, I commit you to God.

Your true, plain, and loving friend,

From Eton College this 7th of October, 1616. JO. HALES.

This letter has been printed in the General Dictionary, vol. v. p. 702.

III.

J. WELLES TO MR. HENRY BRIGGS IN MERTON COLLEGE IN OXFORD.

Good Mr. Briggs,

I have received your letter, and perceive that the errors grow through your man's negligence: I will collect as many as I can hear of: for the other point I will rest till your own time for further satisfaction. And now that you have made me so skilful as to make logarithms by help of others, I am grown ambitious to make them originally. To this purpose, reading my Lord of Merchist. Appendix, I have fastened upon his med. geomet. et arithmet. between 10 and 0, and conferring with Mr. Gunter, he approved of my labour, and hath encouraged me to proceed, and I have already m[ade] 34 several roots of 17 places in each, and for my better assurance that I have done right, desire you to send me word how far I shall need to go, and what the last number and logarithm will .... or of an easier way, if you know any.

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This day the parliament is dissolved by proclamation, to the great grief of all honest hearts. I pray God bless the king and us all with peace and prosperity: this 9th of January, 1621.

Your ever loving friend,

J. WELLES.

J. Welles, the author of Sciographia, or the Art of Shadows, (8°. London, 1635,) was storekeeper at Deptford, and is one of the persons by whom Gellibrand was induced to complete and publish the Trigonometria Britannica. He appears from this letter to have assisted in the calculations for the Arithmetica Logarithmica, and there is the copy of another in the Savilian library, in which Briggs begins by saying, "Good Mr. Welles, I thank you for your kind letter, which "I received by your son, and my numbers enclosed. Since I "had them, I have made them a little more perfect in those "places which were blotted; and if you please to mend them, "and go on a little further, it is done thus," &c. &c. The letter then goes on to exemplify the rules which he lays down in the twelfth chapter of his Arithmetica Logarithmica. Wood (in his Ath. Oxon. vol. iii. 1155.) says, that J. W. "was much "valued for his mathematical sufficiencies, by Briggs, Gunter, "Gellibrand, Oughtred, &c."

IV.

Martin Briggs to his uncle "Mr. Henry Briggs, mathemati- "cian at Merton College in Oxford," dated 4th of Feb. 1628. —This contains nothing, except excuses for his negligence in not sending an earlier letter, "having neither news nor occa- "sion of business to write of."

V.

W. ROBINSON TO REV. W. OUGHTRED.

Sir,

I have sent you here enclosed, the proposition, with its diagram, not to conceive it but me; for

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all Euclid (I know) is most facile unto you, but an ignorant scholar is troublesome both to himself and master, yet as much as concerns this present question, I think I have sufficiently cleared my meaning, to wit, my ignorance of the thing I seek for; for a clear proposing of an obscure and unknown object repugn not. I have diligently read your learned and judicious papers, and cannot but much admire your piercing judgment into the abstruseness of the logarithms; for I cannot but think (by the little glimpse which your paper hath given me) that the ampleness of their use, their facility in practice, and the variety of compendious ways through all trigonometry are so solely in your breast, that unless you have imparted them, all other books that shall come forth of that nature will be defective; and therefore do wish, with all my heart, that you would make the world partaker of your labours, both to prevent the crude wit of some that will perhaps obtrude their books upon us, and to enrich the world with so rich a jewel, as it never had yet in my knowledge in the mathematical science.

Your first rule, e logarithmo minore cum radio, &c. I cannot hit on in this particular case: I miss in this, that being to work by a difference and not by a sum, I suppose consequenter that having subtracted the greater log. sc. of 50. s. 9884239665. from the lesser with the radius, i. e. from 1.94275960547, the remainder will be 95433420882, whose arch is 20. 27. which subducted from 90, the excessus is 69. 33. whose half is 34. 46′. 30″. But why you add sinubus duobus adde logarithmum majorem and 0.3010300, I conceive not, unless I must also have the semissis aggregati as well as the excessus, which cannot be: your rule speaks in that disjunctively, as indeed it must, so that although semissis aggregati et excessus

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be duo arcus vicarii, yet being not to be taken conjunctively but disjunctively, as the particle vel imports, why I should add logarithmum majorem, and 0.3010300 sinubus duobus—to two sines, is the thing I understand not, and therefore must crave your patience to clear me from my own mist; for somewhat there is that I do mistake at. Thus as you see, being bold, if not too bold with you, I shall ever rest,

Jan. 12. Yours to serve you,
W. R.

VI.

OUGHTRED'S ANSWER TO ROBINSON.

My duo vicarii arcus for addition are bini semisses arcus aggregati, and for subduction are bini semisses arcus reliqui. Wherefore in the rule I say semissis aggregati vel excessus sinubus duobus. Peradventure you may think I should more plainly have said (instead of sinubus duobus) sinui duplicato: but I had respect to the manner of working.

The note of multiplication is $\times$. The note of proportion wrought is twice two points set between the two rationes. Division is wrought by setting the divisor under the dividend with a line between them.

Trianguli sphærici duo latera sunt PZ et PS, basis autem ZS.

$$R : s.PS :: s.PZ : om = \frac{s.PS \times s.PZ}{R}$$

$$R : sco.PS :: sco.PZ : lk = \frac{sco.PS \times sco.PZ}{R}$$

$$sco.ZS \pm \frac{sco.PS \times sco.PZ}{R} \text{ sic invenitur per reg. 1.}$$

$$sco.ZS : \frac{sco.PS \times sco.PZ}{R} :: R : s.\text{ arcus prosthaph.}$$

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$$\text{Quare}\frac{sco.\text{PS}\times sco.\text{PZ}}{sco.\text{ZS}}=s.\text{ arcus prosthaph.}$$

$$90\pm\text{area }\frac{sco.\text{PS}\times sco.\text{PZ}}{sco.\text{ZS}}\text{ dicatur arcus}\left\{\begin{matrix}\text{Z}\\ \text{X}\end{matrix}\right.$$

$$\begin{matrix}\text{Quare}\\ \text{per reg.1.}\end{matrix}\left\{\begin{matrix}2s.\tfrac{1}{2}\text{Z}+sco.\text{ZS}+0.3010300-2\text{R}=\text{sn sum.}\\ 2s.\tfrac{1}{2}\text{X}+sco.\text{ZS}+0.3010300-2\text{R}=\text{sn diff.}\end{matrix}\right.$$

$$\text{Et quia est om}\left(\frac{s.\text{PS}\times\text{PZ}}{\text{R}}\right):\text{R}::\text{sn}:sco.\angle\text{P}.$$

Et si e tribus terminis datis secundus dividat primum, et quotus dividat tertium, quotus posterior erit quartus proportionalis: erit $\frac{s.\text{PS}\times s.\text{PZ}}{\text{R}^2}\Big)\frac{\text{sn}}{1}\Big(sco.\angle\text{P}$. Quare per logarithmos

$$2s.\tfrac{1}{2}\left\{\begin{matrix}\text{Z}\\ \text{X}\end{matrix}\right\}+sco.\text{ZS}+0.3010300(-2\text{R})-s.\text{PS}-s\text{PZ}$$

$$(+2\text{R})=sco.\angle\text{P}.$$

Ergo (interpretor etiam illa superiora, arcus Z et X, ad ratioci[ni]um adhibita) si e summa sinuum complementorum laterum tollatur sinus complementi basis; et sinus reliqui arcus addatur vel detrahatur gradibus 90 (prout, &c.); et arcus compositi vel residui dimidiati sinubus duobus addatur sinus complementi basis, et 0.3010300; et ex aggregato tollatur summa sinuum laterum; restabit sinus complementi anguli quæsiti.

I will by word of mouth both demonstrate my two rules, and also shew this kind of ratiocination and analytical deduction according to the reason and ground of the former rule, which I doubt not but you will take delight in, seeing (as I perceive) you desire to mount above the level of ordinary artists, and to look into the foundations which support the structure of these sciences. But my art for all such mathematical inventions I have set down in my Clavis Mathematica, which therefore in my title I say is tum lo-

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gisticæ cum analyticæ adeoque totius mathematicæ quasi clavis, which if any one of a mathematical genius will carefully study, (and indeed it must be carefully studied,) he will not admire others, but himself do wonders. But I (such is my tenuity) have enough fungi vice cotis, acutum reddere quæ ferrum valet, exsors ipsa secandi, or like the touchstone, which being but a stone, base and little worth, can shew the excellence and riches of gold.

This answer is printed from what Oughtred has written upon Robinson's letter. The only liberties taken with it are, the insertion of : between the terms of the ratios, instead of the single point which is used in the MS.; the substitution of the negative sign for + before $s.\mathrm{PZ}$ in lin. 10, and the addition of the decimal point where it was omitted. These may have been corrected in the fair copy, which was sent by Oughtred to his correspondent, and he may likewise have then given a more distinct description of his method, which is so worded, in the preceding notice, as not to convey a clear idea of the grounds upon which it proceeds. His first proportions would, at present, only lead from the real demonstration, which depends upon the following properties: $cos.\mathrm{P} = \frac{cos.\mathrm{ZS} - cos.\mathrm{PS} \times cos.\mathrm{PZ}}{sin.\mathrm{PS}.\ sin.\mathrm{PZ}}$; but the numerator of this fraction $= \left(1 - \frac{cos.\mathrm{PS}\ cos.\mathrm{PZ}}{cos.\mathrm{ZS}}\right) cos.\mathrm{ZS}$. Oughtred therefore takes the angle of which the sine is equal to $\frac{cos.\mathrm{PS}.\ cos.\mathrm{PZ}}{cos.\mathrm{ZS}}$, and his expression then will be $(1 - sin.\theta) \times cos.\mathrm{ZS} = 2 sin.^2 \left(\frac{90-\theta}{2}\right) cos.\mathrm{ZS}$. If either of the cosines of PS or PZ should be negative, the expression would have become $(1 + sin.\,\theta)\ cos.\mathrm{ZS} = 2\ sin.^2 \left(\frac{90+\theta}{2}\right)\ cos.\mathrm{ZS}$. Hence θ is his arcus prosthaphæreticus, and $90 \pm \theta$ his Z and X. The rest requires no explanation.

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VII.

W. ROBINSON TO OUGHTRED.

Rev. Sir,

I cannot but wonder at the indiscretion of Rich. Delamain, who being conscious to himself that he is but the pickpurse of another man's wit, would thus inconsiderately provoke and awake a sleeping lion; for, to let pass Mr. Forster's proofs and your own, which I presume will be too pregnant for him to impugn, he hath so weakly (though, in my judgment, vaingloriously enough) commended his own labour therein in his Preface to the Reader, &c., that any man may see by his mincing terms "of accommodating the instrument," and his acknowledging "no inferior assistance," and the like modifications which do rather imply some *superior assistant*, (for he should be very mean, that were his inferior,) and his own acknowledgment that Mr. Gunter's 4th Projection (p. 64 and 65 of his book called the Sector) did originally come from your labours and invention, which he there seems to prove two or three ways, by all this I say a man may easily see an ambitious desire to challenge the invention to himself, yet checked with his own consciousness to the contrary, for at the most he is but the accommodator, (an easy trifle,) not the inventor, not only assisted but *absolutely instructed* in the demonstrative part thereof; for I know him, by my own trial of him some two or three years since, (which was before my happy acquaintance with yourself,) so weak a demonstrator of mathematical speculations, that he seemed to lament (because I will not say complain) that defect in Mr. Gunter's book, that he had not demonstrated his propositions; which in a professed professor of mathematical science seemed strange unto me, because the demonstrations of most

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of them are obvious to every author (if he understand Latin) that writes of them; and for an instance I tried him myself, to find an angle by knowing the three sides, and that by logarithms according to the way of Regiomontanus set down by Mr. Gunter, pag. 83, 84; but after a day's respite he returned me indeed a pap[er] wrought with the old and natural sines, which I could have done myself, (if I would have troubled myself with multiplication;) and so I thought it good manners to thank him, though for nothing, at least to my purpose or request; but I easily conceived he was not willing to be urged; and I take him to be of a haughty spirit, ofttimes the mask of ignorance; and so we parted. This late book of his I have not seen nor heard of till now, nor much desire, for where he was not such a canis festinans as in this, he is confused, tedious, and involved; he doth not parturire neatly and cleanly, if his conceptions be his own; but, good sir, let me be beholden to you for your Apology whensoever it comes forth, and (if I speak not too late) let me entreat you, whip ignorance well on the blind side, and we may turn him round, and see what part of him is free; which I know you can do with that discretion that it shall not relish of passion at all, and so give no advantages.

I give you many thanks for your last kind letter, and your propositions sent me therein: the bearer's haste is such, that for the present I can say no more; but I will make bold (as opportunity serves) to correspond with your reverence hereafter, and had thought at this time to have enlarged myself in the application of your 16, (pag. 57,) (but Euclid's 36 propos. elem. 2,) to find out the diameter of the earth, insisting upon the supposed proofs or demonstrations of Willebrord Snellius, in his book De Terræ Ambitus

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vera quantitate, which he entitles Eratosthenes Batavus, and have had your opinion whether supposing our standard foot English contains $\frac{968}{1000}$ of a 1000, into which he divides the foot of Holland, and a mile English 5280 foot English, according to a statute 33 of Edward I; also 12 foot Holl. makes one perch Holl. and 28500 Holland perches 1 degree in maximo circulo terræ, all this, I say, being presupposed, whether a terrestrial degree do not contain $66\frac{9139}{10000}$ English miles; for one Mr. Milborne, a minister, (his brother keeps the Greyhound in Paul's Churchyard,) said, it would not contain hardly 63: now methink I should not be deceived in so trivial a work. I had thought to have sent you my whole working, both for the diameter and circumference, according to your 16th, pag. 16, 17, but time will not permit me to finish it as I would, only I will put in the margin the ambitus, diameter and semidiameter of the earth, according to Snellius his excessive labour. Thus, my best affections remembered to you, I shall ever rest

Your ever assured and obliged servant,

June 12. W. Robinson.

In a postscript, Mr. Robinson gives his numbers, and calculates the earth's magnitude from the 36th prop. of the third (not the second) book of Euclid. To do this, he assumes that the top of a mountain, of which the perpendicular height is $1\frac{1}{2}$ English mile, will appear on the horizon of a spectator whose distance is 1° of a great circle of the earth. His diagram is evidently taken from that in p. 252. of Snellii Eratosthenes Batavus; and he adds the following remarks:

" But I have wrought this hastily, and so may err. If you " never read that book of Snellius, I would you would find " leisure to peruse it; for if ever any man went the demonstra- " tive way, (and labour there wants none in him,) he hath " done it.

" This Snellius measured Holland, as if you would measure

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"an acre of ground: all others before him did nothing. His "way being solid, the distance and magnititude of the planets "may be brought to some certainty, and the longitude of "places found out, if any man will be as painful as he for their "country."

From the mention of the dispute between Oughtred and Delamain, it is probable that this letter was written about the year 1630.

VIII.

A letter to Oughtred from W. Alabaster, dated 27th of August, 1633, in which he deduces the quadrature of the circle from the fourth chapter of Ezekiel. This has been printed in the General Dict. vol. viii. p. 80, and did not appear to be worth reprinting in the present collection.

IX.

W. ROBINSON TO OUGHTRED.

Rev. Sir,

But that your letter came upon the very point of my departure out of town, (when commonly businesses come fastest on, and myself indeed not very well,) I had returned you [an] answer whilst I was there; but for those reasons, and some others, I presumed to defer a little, till both mine own occasions and a fit messenger, as now it happens, bring it up unto you. As for the book I sent you, (which I ingenuously confess was out of my true love unto you,) I give you thank for your perusal thereof, for your judgment and plaindealing therein; and that being all I requested at your hands, I rest satisfied, and concerning it there shall not need to pass any more betwixt us. I am glad also you have perused Snellius, who indeed pleased me exceedingly, and I thought, in the reading

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of him, a more exact way (if instruments and artist be both exact) could not possibly be taken than by angles taken with a very large quadrant, and so good an artist and logistic as Snellius was. Upon your letter I borrowed your new book, (but I intend to take your courteous offer of one of them from Mr. Allen,) and find your way, pag. 23, to be exceeding good; (and indeed a curious good way;) but why in your letter you should rather wish the plain Table, I do not see, yet I confess my ignorance herein: being only a book speculator, (such is the condition of my life,) and no practitioner, because I am restrained, may well hide the reason from me, and therefore do submit. In assigning of a degree, I find this difference betwixt you and Snellius, that he makes a degree to contain $353305\frac{785}{1000}$ English feet, you only 351120, (p. 24.) the difference is $2185\frac{785}{1000}$, which though not much in a whole degree, yet I marvel how it should happen betwixt such two good artists. The way by the height of a hill and a tangent, which is Maurolycus' way, I did not propose as to rely upon it in practice; but supposing Snellius his observations to be exact, I thought good to try, the earth's diameter being already given, and the height of a hill also supposed to be 7920 feet, that is, a mile and a half in the perpendicular, (a height high enough,) yet it will not hit, but either varieth the angle at the centre, or the diameter given by Snellius; yet I dare not distrust Snellius his way, it is so demonstrative. Your book of navigation delighteth me exceedingly; and I doubt not but to find in it (for my time of perusal hath yet been but short) the way of the ship exactly taught and something extraordinary, seeing (as I perceive) you have read Snellius his Tiphys Batavus, which was the best that I had read before I read yours; and seeing you have laid open so

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much of the art already, I could wish we might see a Tiphys Aldeburiensis come from the Archimedes thereof; for verily I think omne bonum mathematicum may come from thence, if so you please; and I protest unto you sincerely, were I as able as some, at whose hands you have merited exceedingly, or (to speak more absolutely) as able as willing, I would as freely give you 500*l.* per ann. as 500 pence; and I cannot but be astonished at this our age, wherein pelf and dross is made their summum bonum, and the best part of man, with the true ornaments thereof, science and knowledge, are so slighted. I would, I say, it were in my power as efficaciously as it is in my will, and there should not pass many days ere it were done. I make no question but my lord Marshall [b] is very noble, and intended really to you; but methink there was some neglect that so poor a favour had so bad success. Had I interest about London, (for I see and indeed [it] is most convenient you would not be far from thence,) I would do my best, which I will do notwithstanding, though I can promise nothing but my good-will, which shall not be wanting to the utmost. I shall long exceedingly till I see your Clavis turned into a picklock; and I beseech you enlarge it, and explain it what you can, for we shall not need to fear either tautology or superfluity; you are naturally concise, and your clear judgment makes you both methodical and pithy; and your analytical way is indeed the only way; and if I could speak a word in your ear, (for I would not hinder Mr. Allen's profit,) I could wish your books might be absolute of themselves, without any dependence of instruments, which are either little, and so of no use, or if large, very costly, and at the best (to use Mr. Forster's words, or rather your own by his pen) "they make

[b] The earl of Arundel, to whose son Oughtred was tutor.

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scholars only doers of tricks, or as it were jugglers, &c." revera præstigiis instrumentorum quasi in cortice, magna artis jactura, imo contemptu detinentur yourself truly averreth in your Ep., which I speak not as fearing that your Clavis shall be accommodated to any instruments, but to prevent any other future work of yours, unless withal it be absolute of itself that a man may choose whether he will buy the instrument or no.

As you have brought in a symbolical kind of expression, (which is very neat and good,) and wish also that the decimal fraction were common, and if the decimal integer were brought in also, (but that it would make shrewd havoc amongst all Tables extant to divide a whole circle into ten,) it would be easy. But if division remaining, which is now current, I would have Snellius his terms of complementum and residuum brought in, the one being datæ peripheriæ ad quadrantem differentia, the other, excessus semicirculi ad datam peripheriam: small things, I confess, yet proper and distinct, whereas complementum ad semicirculum is a circumlocution, and it is better that distinct things have distinct names. I am not ignorant that yourself have given a touch of this, pag. 94. ch. 13, of your Trigonometry, but the word *excess* you seem to use in a different sense.

I have light upon your little book of artificial guaging, wherewith I am much taken, but I want the rod, neither could I get a sight of one of them at the time, because Mr. Allen had none left. The nature of this book requires instrumental operation, and therefore is well accommodated thereto. I forgot to ask Mr. Allen the price of one of them, which if not much I would have one of them [a]. My friend in

[a] Or in wood, if any be made in wood by Thompson or any other. W. O.

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London, one Mr. Headlame, dwelling next door but one to the Plough stables, in Lincoln's-Inn-Grange, will pay him for it, or any thing I send for; he is the man I use for all my letters, to and fro; so that if you please to write and send it to him, he is to come this vacancy into Lincolnshire about business of his own, and therefore [I] could wish your leisure would permit you to bestow a letter upon me. I would gladly hear of any thing you intended for the public view, and I would entreat you to leave no posthuma behind you.

As for Mr. Gunter's mean diameter, I conceive him thus:

A wine vessel being in $\left\{\begin{array}{ll}\text{length} & 66\\ \text{the diameter} & 38\end{array}\right\}$ inches containeth 324 gallons as is supposed by experience.

324	25105450102	content.
66	18195439355	length.
Sum	43300889457	
$146\frac{.232}{1000}$	21650444728	mean diameter.

"$66 \times 38 \times 38 : 324 :: LDq : C$ Quare

$$\frac{66 \times 38 \times 38}{324}\Big)\, LDq \Big(\, C \text{ vel } \frac{66 \times 38 \times 38}{324} : L :: Dq : C$$

At $\dfrac{324 \text{ in.}}{66 \times 38 \times 38}$ est 0.0034. Quare $LDq \times 0.0034 = C$ but in feet and tenth parts 5.8746.

W. O."

Then as 324 to 146, so 38 the diameter to 17.15 the guage point.

Then as the guage point to the mean diameter of any vessel, so the length of the same vessel to a fourth. And

So this fourth number to the content in gallons

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and parts of a gallon according to London wine measure.

This is only to shew whether I conceive him or no; but of the truth thereof I will better inform myself by your pretty book; for now time will not give leave; and besides, I am sometimes so troubled in my head, since a quartan ague that I had, that I much fear a vertigo, for which, if you be as good a physician as mathematician, I would earnestly crave your best advice. It is not come to such a height as yet, though I have shrewd indications of it; but it oft puts me in mind of Principiis obsta sero medicina paratur, &c. and therefore propose it to you, that if either by yourself or some learned friends I may receive advice, I shall think myself much beholden. It comes certainly from my stomach [b]. * * * * * * I have read somewhat in physic myself, and my natural genius led me to physic and mathematics, in both which I should have had some insight, if a more serious calling had not diverted me. God's will be done; my life is solitary, my companions books, my liberty retiredness, so that how I should be cured of this infirmity I well know not, but refer all to his blessed will, comforting myself with that, Placens Deo factus dilectus—raptus est, ne malitia mutaret intellectum ejus, aut ne fictio decipe[re]t animum illius, Sap. 4. com. 10. 11. and indeed the whole chapter may make any man resigned. Thus, dear sir, my best love remembered to you, I shall ever be ready with my best endeavours to do you service, and remain

Your much obliged friend,

June 11. W. Robinson.
(Received June 28th.)

[b] The writer here details medical circumstances which are not calculated for publication.

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Oughtred's Navigation, being "an addition unto the use of the instrument called the circles of proportion," was published in 1633, which marks the year in which this letter was written. From the latter part it appears that the writer was a clergyman: one of the name of Wm. Robinson was archdeacon of Nottingham from 1635 to 1660.

The first part as far as "a different sense," p. 17, is printed in the General Dictionary, vol. viii. p. 82.

X.

DERAND TO OUGHTRED.

Remitto, doctissime domine, quam nuper accepi nobilissimi Domini Caroli Cavendish manibus, fabricatam a te et quidem eruditissime, earum quas ad illum miseramus propositionum demonstrationem. Hanc prima fronte admirantibus nobis nihil occurrit quod difficultatem pareret, at ubi diligentius in eam incubuimus, nonnulla quæ non satis probata suspicamur occurrerunt, quæ tibi proponenda, ut tandem pro ingenii tui . . . unditate perferas remittenda duximus. Sunt autem illa quæ sequuntur.

* * * * * * * * * * * *

Vester servus humillimus,
FRANC. DERAND.

Oct. 1634.

The remainder of this letter is omitted. Derand's writing is in parts illegible: the difficulties, likewise, which he states are insulated and refer to a demonstration of which no copy has been found.

XI.

OUGHTRED TO DERAND.

Accepi literas tuas, doctissime domine, 5°. Non. Octob. stylo vetere, Aldeburiæ nostræ, ubi parœci munere fungor. Quibus lectis miratus sum te, cum

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quod in demonstratione mea nervosius est percipias, de ipsa ἐκθέσει καὶ παρασκευῇ ambigere. Ut igitur dubitationibus tuis satisfaciam, eaque quæ non satis explicata videbantur elucidem, sciendum primo est, cum depingendæ in plano sphæræ duo sint modi, unus rectus per incidentias ex ipsis circulis in subjectum planum perpendiculares, peripherias exhibens ellipticas, quod Analemma dicitur, alius obliquus et opticus per radios visorios, me in hac demonstratione utramque contendere et conciliare; dexterior enim schematis pars tota est projecturæ rectæ; sinisterior, ut plurimum perspectivæ.

* * * * * * * * * * * *

Atque hæc ad quæstiones tuas circa demonstrationem meam responsa sunto. Valeat in Christo dignitas vestra: cui humillimum se servum profitetur.

Guil. O[ughtred.]

3 Octob. 1634.

Oughtred's answers are clearly written, but they could be of no use without the objections to which they are directed. Neither in this nor in Derand's preceding letter is there any diagram by which the reasoning and letters of reference might be elucidated.

XII.

DERAND TO OUGHTRED.

Eruditissime Domine,

Recepi dominationis vestræ literas simulque dubiorum illorum solutionem, quæ mihi vester in solvendis mathematicis problematibus agendi modus, doctissimus ille quidem sed apud nos non ita receptus, procreaverat.

Ubi enim hypothesim semel aliquam ad propositam quamlibet demonstrationem assumpsimus, illam non

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patimur quamdiu probationum diffluit series immutari. Et si quando contingat eam facilitatis majoris gratia aliasve ob causas variari, id admittimus quidem, at ita tamen ut simul etiam lectorem, ne factæ mutationis inscius deludatur, ut nobis dum vestras rimaremur demonstrationes contigit, admonemus. Et certe si dominatio vestra prioribus suis in literis de variata nos demonstrationum suarum ratione fecisset certiores, haud dubium est, ni fallor, quod illarum secreta omnia primis conatibus, ut et secundis fecimus, mutatis ad mentem vestram ideis penetrassemus. Cæterum gratias agimus dominationi vestræ immortales quod, quemadmodum suo illo labore ac studio nobilissimi domini de Cavendish votis plenius satisfecit, sic nostra etiam cumulatissime adimplevit, vale.

Dominationis vestræ
Obsequentissimus in Christo,
Servus FRANC. DERAND.

Parisiis 8 Jan. 1635.
Portatæ sunt ad me hæ literæ Aldeburiæ degentem Feb. 27^o.

This letter is printed in the General Dictionary, vol. viii. p. 81.

XIII.

SIR CH. CAVENDISH TO OUGHTRED.

Worthy Sir,

I thank you for your little book, but especially for the way of calculating the divisions of your guaging rod. I wish, both for their own sakes and yours, that the citizens were as capable of the acuteness of this invention, as they are commonly greedy of gain, and then I doubt not but they would give you a better recompense than I doubt now they will. I deferred

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to write to you until I heard from Mr. Derand, which I have now done, and send you here inclosed his letter to you, wherein I perceive he is now fully satisfied of the perfection of your demonstration. I also received a letter from him, in which he highly commends the sharpness of your invention in that demonstration; he writes also, that your Clavis is in great estimation amongst the mathematicians at Paris. I have no more to trouble you with at this time. And so wishing you all happiness, I rest

Your assured Friend,

CHARLES CAVENDISH.

Wellbeck, Feb. 11, 1635.
init.

This letter is printed in the General Dictionary, vol. viii. p. 81.

Sir Charles Cavendish, whom Clarendon justly describes as "a man of the noblest and largest mind," was a younger brother of William, duke of Newcastle. An account of him may be seen in Collins's Historical Collections, (p. 24.) and Lloyd's Worthies, (p. 672). It may also be mentioned that being a great admirer of Des Cartes he endeavoured, about 1640, to bring him and Mydorge over to England, that they might be settled there under the patronage of Charles the First. This admiration, however, did not degenerate into unjust partiality, for Wallis tells us that he convinced Roberval of Des Cartes having, in the theory of equations, been indebted to what had been previously invented by our countryman Harriot. Kennet in his account of the Cavendish family, (p. 96.) mentions a number of papers on mathematical subjects then in the possession of Moore, bishop of Ely, and which he describes as having been written by sir Charles Cavendish, brother of William, earl of Devonshire; but in this he may have been led into error by the similarity of the name. It is more probable that they belonged to the writer of the present letter, who was distinguished for his attachment to science, than to a young man, who was unfortunately killed in the civil

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wars before he was three and twenty, and who had spent the last five years of that short life either in travelling abroad, or in active military service.

XIV.

ROBINSON TO OUGHTRED.

Rev. Sir,

Having intricated myself in a difficulty, (by reason I see reason on both sides, and yet know not how to relieve it to my own understanding,) I am forced, as well because I would know the truth in itself, as also because it is grounded in one of your books, to make my recourse to you, and although the effect hereof (speaking of the practice) would not be worth your labour and mine, yet in the speculation, whereby I shall see mine error, 'twill be worth both, which makes me thus bold to trouble you. The matter is this: undertaking for my pleasure the making of a dial for a north plane, declining 12° 16′ towards the west, and reclining 48° 50′ from the zenith southward; I calculated all things according to your rules set down in the 149th page of your Art of Dialling, and so we stood securely; but afterwards, out of curiosity—no distrusting—I calculated over again, (though somewhat a longer way,) according to Mr. Gunter's canons, I found all things agree well enough, (for I weigh not the difference of some few minutes, and scarce was there that,) save only the distance of the substile from the horizon, which according to you, was 31° 42′, but according to him, only 13° 13′, for though you both agreed to make the distance of the hour of twelve to be 80° 42′ from the horizon, yet Mr. Gunter made the distance of the substile from the hour of twelve to be 67° 29′, and therefore from the horizon can be distant only 13° 13′, as I said before.

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The calculating of the hour lines increased the difficulty, for the calculation of the hour of twelve still gave me 67° 30′, which was Mr. Gunter's dist. betwixt the substile and twelve, and yourself agreeing with him in the distance of twelve from the horizon, and so that (to wit 80° 42′) being immutable betwixt you, and the way of calculating the hour lines common to both, yea to all men, I was at a blank. On the other side, having calculated the height of the stile according to you, (and consequently did not mistake in neither of your bases,) I found the height of the stile to be 81° 19′, which is his also; this agreed upon by both sides; by it and by the s. of declination and cos. of the latitude of the place, to wit 52° 48′, (both which are first principles given,) I found the distance of the substile from the horizon to be 31° 42′, wherein I confess I see no cause of error; for I observed all your cautions as well as I could. I rested not here, but tried another way, which your way gave me light unto, which confirmed your ways both for the stile's height and distance of the substile from the horizon; and thus I wrought:

$$\left.\begin{array}{l}9.3272811\\ 9.7814675\end{array}\right\}\text{the}\left\{\begin{array}{l}\text{sin. of } 12^\circ\ 16' \text{ declination.}\\ \text{cos. of } 52\ \ 48 \text{ latitude.}\end{array}\right.$$

$$\underline{\underline{9.1087486}} \text{ cos. of } 82^\circ\, 37' \text{ whose}\left\{\begin{array}{l}\text{s. } 9.9963840\\ \text{t. } 10.8874562\end{array}\right\}\text{is}\left\{\begin{array}{l}1^{um}\\ 2^{dum}\end{array}\right\}\begin{array}{l}\text{inven-}\\ \text{tum.}\end{array}$$

Then

$$\left.\begin{array}{l}9.9899698\\ 9.8802654\end{array}\right\}\text{the}\left\{\begin{array}{l}\text{cos. of } 12^\circ\ 16' \text{ declination}\\ \text{cot. of } 52\ \ 48 \text{ latitude}\end{array}\right.$$

9.8702352 the tang. of 36 34, which is your 1st base,
to which I add,

$$\begin{array}{ll}\text{the recli.} & 48^\circ\ 50' \\ \text{and it makes} & 85\ \ 24\end{array}\text{ whose}\left\{\begin{array}{l}\text{sin. } 9.9985988\\ \text{cos. } 8.9041685\end{array}\right\}\text{is}\left\{\begin{array}{l}3^{um}\\ 4^{tum}\end{array}\right.\left\{\begin{array}{l}\text{inven-}\\ \text{tum.}\end{array}\right.$$

$$\left.\begin{array}{l}9.9963840\ 1^{um}\\ 9.9985988\ 3^{um}\end{array}\right\}\text{invent.}$$

$\underline{\underline{9.9949828}}$ the sine of 81° 19′ the height of the stile above the substile or plane

$$\left.\begin{array}{r}10.8874562\\ \underline{8.9041685}\end{array}\right\}\text{the}\left\{\begin{array}{l}2^{d}\\ 4^{th}\end{array}\right\}$$

9.7916247 the tang. of 31° 45′ the distance of the substile from the horizon, whose complement, to wit 58° 15′, is the distance of the substile from the line perpendicular to the horizon.

Then

$$\left.\begin{array}{r}9.8766784\\ \underline{9.3373112}\end{array}\right\}\text{the}\left\{\begin{array}{l}\text{sine of reclin.}\\ \text{tang. of declin.}\end{array}\right\}$$

9.2139896 the tang. of 9° 18′ the distance of the hour of 12 from the foresaid perpendicular,

to which if you add 58 15 the former complement,

$$\left.\begin{array}{ll}\text{it makes Mr. Gunter's} & 67\ \ 33\\ \text{if substract, yours} & 48\ \ 57\end{array}\right\}\text{distance betwixt the substile and the hour of 12.}$$

But my chief request is, (abstracting from this third way,) to understanding how it comes to pass that Mr. Gunter and yourself should differ in placing the substile line, (a main matter, especially where the difference is so great,) agreeing in all the rest, and yet you take nothing for granted but either first principles given, (as declination and latitude,) or, if a thing inferred, (as the stile's height sc. 81° 19′,) yet it is his also even to a minute; and when I examine the diagram, I can find no cause of error, neither (as I suppose) do I mistake midnight for noon; so that which way it should come thus to pass, I do not know. I have drawn the diagram [b], that you may better see my mistaking, if any have befallen me, and no doubt but there hath, otherwise I could not make such difference betwixt two such artists, and therefore have either missed in my calculation, or application of things rightly done, though I confess I see it not. For my close, I will once again earnestly entreat you, that you be rather diffuse in the setting forth of your

[b] The diagram is a projection like that in p. 26. of Gunter's book "Of the use of the lines "of numbers," &c.; and as it is but roughly drawn, there seems to be no adequate advantage in the insertion of it.

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English mathematical Clavis, than concise, considering that the wisest of men noted of old, and said stultorum infinitus est numerus, these arts cannot be made too easy, they are so abstruse of themselves, and men either so lazy or dull, that their fastidious wits take a loathing at the very entrance of these studies, unless it be sweetened on with plainness and facility. Brevity may well argue a learned author, that without any excess or redundance, either of matter or words, can give the very substance and essence of the thing treated of; but it seldom makes a learned scholar; and if one be cap[ab]le, twenty are not; and if the master sum up in brief the pith of his own long labours and travails, it is not easy to imagine that scholars can with less labour than it cost their masters dive into the depths thereof. I am not of their opinion that would have mysteries in the world, (divine mysteries, which God himself hath locked from us, excepted,) for ignorance is the punishment and consequence of sin; and it behoveth us to take away from each other this veil, which sin hath drawn between our eyes and the truth of things; "so shall men imitate the angels, every su-"perior illuminating and perfecting his inferior, which "redoundeth to the honour and dignity of them both, "but more of the giver than receiver." The abyss of these sciences is inexhaustible, and much is lost that our ancestors knew: much [is] yet unknown to either; and therefore all the help that human wit or industry can afford is all little enough; and when all is done, there will be a terra incognita for mathematicians of after ages to sail into. Thus, good sir, you see I am free and bold, because loving and confident; and if you will be pleased to take it, I will count it pardon sufficient: and whatsoever issueth from you, as a child of

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your brain, I desire it may lodge with me, where it shall have the best welcome that affection can give it.

Your loving and true friend,

Lincoln, July 2, WILLIAM ROBINSON.

(1636, Brought to me July 16, at night.)

The latter part of this letter (beginning with the words, "I will once again," &c. p. 26.) is printed in the General Dict. vol. viii. p. 82.

XV.

DERAND TO SIR CHAS. CAVENDISH.

Monsieur,

Je m'estimerois tout-a-fait digne de blame si de tems en tems je ne mettois la main a la plume pour vous rendre mes devoirs, et vous saluant par mes lettres vous donner les assurances que l'estime que j'ai fait de votre connoissance et de vos merites, depuis le tems que j'ai eu l'honneur de vous pratiquer, ne recevoit en mon ame aucune diminution, ains[i] que j'irai la cultivant toujours de plus en plus comme chose de la quelle je fais un singulier etat. Oserai-je, monsieur, vous divertir de vos plus serieuse occupations par un petit probleme, la solution duquel si elle se trouve geometrique aura de tres belles suites, et . . en des matieres sur lesquelles il y a longues années que les mathematiciens travaillent : le voici.

In dato angulo acuto BAC ducta ab uno latere AB ad aliud latus AC perpendicularis BC ita dividatur, ut per punctum divisionis D protracta ab angulo A usque ad lineam BE prædicto lateri AC parallelam ac per punctum B transeuntem, recta AE duplum lateris AB inter D et E puncta contineat.

Je voudrois, monsieur, avoir quelque autre piece plus

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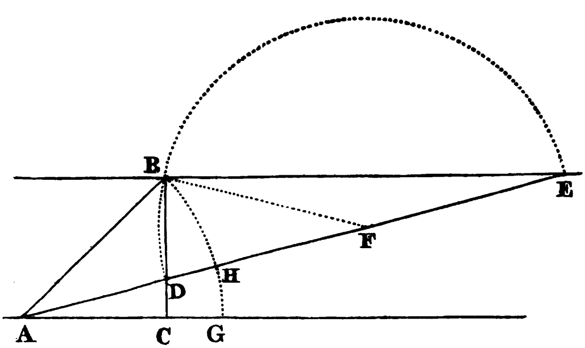
digne de votre bel esprit que la presente; car j'aurois un singulier contentement d'avoir rencontré quelque chose, en la nouveauté et rareté de laquelle vous puissiez recevoir quelque agreable divertissement en vos meilleures occupations, lesquelles vous me pardonnerez, s'il vous plait, si j'interrompe peutetre trop fautement: c'est le desir que j'ai de vous temoigner que je vous suis, Monsieur,

Serviteur tres humble et tres obeissant,
FRANCOIS DERAND.

A Paris ce 1 Aout, 1637.
This was delivered to me at Aldebury from
Sir Charles Cavendish, Octob. 12, 1637.

On the side of this letter Oughtred has written the following solution of the problem. It does not meet the real difficulty; because it assumes the trisection of an arc; but it is evident, from the annexed date, that it was written out as soon as Derand's letter was received from sir Chas. Cavendish.



Centro A describatur arcus BG, cujus triens sumatur GH : et ducatur ADHE. Nam diametro DE fiat semicirculus DBE : et ducatur ipsius Radius:
Erunt anguli EBF=BEF=DAC
Ang. BFD=2BEF=2DAC=BAD.

Octob. 12°. 1637,
Stylo Anglico vetere.

XVI.

Joannes Huniades Hungaro-Transilvanus, 15th of December, 1637. He had translated some German work on natural philosophy into Latin, and sends a copy to Oughtred: the letter may be seen in the Gen. Dict. vol. viii. p. 83, and is not worth reprinting. The only part of any interest is the following postscript:

"Mr. Foster, our lecturer in astronomy in Gresham "College, is put out because he will not kneel down at "the communion table, when he takes the commu- "nion. A Scotsman, one that is verbi bis minister, is "now lecturer in Mr. Foster's place. Saluta nomine "meo Wilhelmum Oughtred juniorem, quæso."

XVII.

OUGHTRED TO ELIAS ALLEN.

Good Mr. Allen,

I have here sent you directions (as you requested me, being at Twickenham) about the making of the two rulers, part whereof I have noted in the sheet you left with me, which I have here inclosed, and part I will here deliver.

1. For setting the degrees on the staff. Divide the staff from the end next your eye, to the place whereabout the pinnicide or sight is to stand, into 26 equal parts, obscurely, whereof the first 15 next the end are for the radius 100000, according to which the tangents of degrees from 0 to 60 must be measured; but those degrees are to be noted with the figures of their complements, beginning with 30, which is the true place

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of the pinnicide, and going forward to 90, which will be at the very end of the staff next your eye.

2. For setting the degrees on the transversary. The radius, according to which the tangents are to be measured, must be equal to the space between the pinnicide of the staff and the end; that is, the tangent of 60 deg., viz. 173205 parts, whereof 15 of the former obscure divisions contained 100000; and, if you please, you may after 30 set as many degrees as the side will hold.

3. The lines of numbers, sines, and tangents are to be set on the transversary in the same manner as they are set on the staff in Mr. Gunter's cross staff; and that the divisions may be the larger, you may (if you think good) make the transversary three quarters of the staff's length.

4. The divisions of the line of latitudes, and of the line of 100 equal parts, on the fourth side of the staff, must not be set to the edges, (as the other divisions were,) but in the middle, close together, that the one may shew the other. The rest is plainly enough set down.

5. The line of equal parts, on the fourth side of the transversary, from the unit line to the end of the ruler, is to be divided into parts $17\frac{2}{3}$; viz. at 10, 9, 8, 7, 6, 5, 4, 3, 2, 1, 10, 9, 8, 7, 6, 5, 4, 3, $2\frac{1}{3}$, whereof the ten first, from 10 to 10, are equal to the space from 1 to 1, in the line of numbers: as also it appeareth in the circles.

6. The sockets must be so made that they may easily be taken off, and that the transversary may stand on the right hand of the staff, so that both the lines of degrees may be close together.

I think you will make no doubt of any thing about the rulers. I would gladly see one of them when it is finished, which yet I never have done. Now I will

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put you in mind of my compasses; but so for this time take my leave; and with my love and best wishes to you and yours, I rest.

Your very loving friend,

Aug. 20, 1638. WILLIAM OUGHTRED.

Elias Allen was an instrument maker who lived near St. Clement's church in the Strand. Inclosed in the letter is an impression of a copper plate two feet long, and at the end of the letter itself are some columns of numbers, at the head of which is written, "The sight of the transversary;" but the contents in both cases are only the quantities arranged as on Gunter's scale for the equal parts, sines, tangents, and line of numbers.

XVIII.

EWART TO OUGHTRED.

Good Mr. Oughtred,

This is only to let you know I do not go into Nottinghamshire, so that when your occasions will give you leave, I shall be glad to see you at Twickenham. I have done to your eighth problem, and understand them well; but to that I can say nothing, it is so difficult; for I know not why

$$\frac{BK \times BD \pm BF^2}{2BF} = BC \text{ and } CF.$$

If you can make this problem plain by letter, I pray you do; if not, I will expect till you come; to whom, with your wife, I commend me very kindly.

Your loving friend,

20th July, 1639. N. EWART.

(I received it Tuesday, July 23.)

This refers to the 20th chapter of Oughtred's Clavis (p. 66. in the edition of 1631). Oughtred has written a full illustration

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of it on the letter, but there is nothing, which at present can be considered as a difficulty in the problem, and it would be useless to print the explanation.

XIX.

W. GASCOIGNE TO OUGHTRED.

Sir,

Amongst the mathematical rarities these times have afforded, there are none of that small number I (a late intruder into these studies) have yet viewed, which so fully demonstrates their authors' great abilities as your Clavis, not richer in augmentations, than valuable for contraction; that forceth me to censure myself presumptuous in this action, and this tedious, that I have not already related the occasion. The choicest foreign wits within these thirty years as Galileus, Scheiner, Kepler, Hortensius, and Monsieur des Cartes, have so diligently pried into perspectives, that it may credibly be thought, that their rarest use is already known; yet so it is, I have either found out, or stumbled on, which indeed they have always stumbled at, a most certain and easy way, whereby the distance between any the least stars, visible only by a perspective glass, may be readily given, I suppose to a second; affording the diminutions and augmentations of the planets strangely precise, as also their centres; stretchable to the invention of the moon's true parallax of altitude by its own body, and of the inclination of one star to another of a well known site; and able to bring sufficient aid to your aged eyes to find all requisites (according to yours of navigation) at a large distance for searching the earth's diameter. It is a novelty, capable of such frequent use, that before it travel to

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other able judges, may I receive that favour it shall undergo your experiment and censure? I bestow only part of that time in these studies, which other gentlemen, our neighbours, spend in hunting, and want that great help an able artist might afford; and believe that you will find those errors I see not. If you please to grant my request, I may be readily informed by a letter sent by the northern post, or left at the Bear in Basinghall street, with the Wakefield carriers, directed to my father's house in Middleton, near Leeds, where remains,

Sir,

Your troublesome unknown friend,

Decemb. 2, 1640. WILL. GASCOIGNE.

This letter was printed in the Gen. Dict. vol. viii. p. 83.

William Gascoigne was the lineal descendent of sir William, the celebrated chief justice of king Henry IV. Middleton, from whence he dates his letters, was in the parish of Rothwell. He was born about 1620, but the exact time is not known; his baptism not having been found in the parish register, nor his name, although he speaks of his residence at Oxford, in the matriculation books. The mighty promise of his early talents was cut short in 1644, when he fell with other loyal subjects of Charles I. at Marston Moor. Townley, in the Phil. Trans. (vol. ii. p. 457), vindicates his claim to the invention of the micrometer; and the account, which he gives in the following letter of the discovery, is a very valuable addition to the reports that have hitherto been published of it. Derham, in the Phil. Trans. (vol. xxx. p. 603), states him to have been also the first who adopted the use of telescopic sights.

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XX.

W. GASCOIGNE TO OUGHTRED.

Most honoured sir,

Your letter hath emboldened me to present this to your view; if the contents deserve your approbation, I shall not after it fear the most prying eye that may examine it. Your belief that there is in all inventions aliquid divinum, an infusion beyond human cogitations, I am confident will appear notably strengthened, if you please to afford this truth belief, that I entered upon these studies accidentally after I betook myself to the country, having never had so much aid as to be taught addition, nor the discourse of an artist (having left both Oxford and London before I knew what any proposition in geometry meant) to inform me what were the best authors; nor being rich enough in language to understand perfectly any tongue except my mother's, having had two years' interim between the school and university, where I dare not say that I learned more than how those lived, that increased their knowledge, and to know my own wants by others' wealth, and to be sensible that the judgment at Babel was no trifle. These difficulties might have produced despair, had I not been furnished with an ardency to vanquish them, (at least in some small measure,) and continually refreshed with a little increase, whereof the whole stature is so dwarfish, that I must intreat all your expectation of great matters (from one that is thus every way unlikely, and whose sole aim and end was his own pleasure and satisfaction) to be set apart; which hoping you will perform, I proceed.

A straight course suits not well with refraction, the paths whereof are such by-ways, that the very industri-

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ous Kepler, p. 113. Paralip. in Vitellio. professeth, in genuina hujus rei causa directe et a priori demonstranda hæreo. And thus marginal notes it, Dic quibus in terris, et eris, &c. In p. seq. from Vitellio's gross observation he labours, by a tedious and irrational multiplication of calculations to introduce true elements thereof. Had he in this used his usual observancy, he would have followed his old masters to whom he thus in pag. 95 testifieth his affection: plurimum namque amo analogias, fidelissimos meos magistros, omnium naturæ arcanorum conscios: and would (with an admiration of this harmony) have said, what upon another occasion he testifieth in p. 650. Epit. Astron. Omnis motus naturalis vel artificialis, in quem vel eadem vel analoga concurrunt principia, dispensatur per sinus angulorum: præcipue vero et evidentissime, motus vel nisus brachiorum in libra et statera. If in the first scheme (in one of these enclosed papers) *n e* be the handle (of a pair of balance) *e* the centre whereon the balance *g e a* moves, *g* one weight, *a* another, differing (according to him) as *n p*, *n f*; which are shewed in the handle and its continuation, by *a p*, *g f*. if the weight at *g* be supposed to be let fall into water, according to its proportion to the equal solid of water will be the ascension of the end of the balance at *g*, which if to *k*, *a* will descend to *c*; so that $c : a :: nd : np$, $k : g :: nm : nf$. and the angle of mutation gek $(=aec)=mek$ $(=ced)$—feg $(=pea)$ is here not unlike the angle of refraction. For if we suppose (as I made this observation here under) at *g* a plate of well polished brass, *gf*, *fe*, a rectangle instrument of a competent size, *g e a* a ray in the free air, after this *qel* a surface of water under which *ef* is placed perpendicular, *g* will ascend unto *k*, and *fg* will be (the water

being to the air as when I made this) to *mk* = *cd*, as *s*.30°. to *s*.42°. 15′. which proportion is the same in all inclinations as is manifest by this table, in which I rather choose 30°, 42° 15′ than 45°, 71° 58′, because here by reason of the greatness of the inclination, and the augmentation of the obscurity of the water answerable to the secants, the brass plates could not be so perfectly discerned.

45°	71° 40′		*s*.45°	*s*.71° 58′
40	59 45		40	59 49
35	50 27		35	50 28
30	42 15		30	42 15
25	34 46	*s*.30° : *s*.42° 15′ : :	25	34 38
20	27 23		20	27 23
15	20 16		15	20 22
10	13 30		10	13 30
5	6 45		5	6 44
feg	*ced*			

If I did not believe this catalogue sufficient, I could have been more curious. For experiment sake I tried and found water 1876, glass 4791 of equal solids: and *c d* in air to *fg* in glass near 52° 35′ to 31° 20′, which is therefore to *h i* in water, as 31° 20′ to 36° 12′, whence should follow that *b* should be to *g*, as 1876 to 2915, and this in all inclinations which is not agreeable to truth, (and that *b* should be to *i* as 1 to 400 according to Galileus, mecan. p. 81.) if the refraction of these could be conjoined with their weights they might have some excellent uses; therefore I have inserted this trial of their weights. Monsieur des Cartes's conceit that the rays of light move more easily in those bodies wherein they tend toward the perpendicular, than in those wherein they make from it, is not for aught I yet see without as great inconveniences as this analogy of the weight. Whether it be the one or the other, yet *cd*, *fg* are the

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true measurers of the refraction which he in his excellent discourse, De l'arc-en-ciel, sets as 250 to 187[c], an unit differing from mine. In all the other draughts every perfect circle is set for this, and its centre for *e*, the superficies, in which *e* is, for *ql*, the lesser angles contained by the ray of incidence (here noted by the black lines) and the perpendiculars to the refracting plane are as *feg*, and the greater as *ced*; all their sines are to be supposed where they want, and the letters in every mentioned variety to be placed in their right site, in which position *SQl*, in scheme 2, denotes a rectangle triangle of glass polished on its three edges, *CBSQb* a ruler parallel to *SQ*, on which ruler *CO*, *BO* are 2 perpendicular plates perforated, through which the ray of incidence *Ae* is received, and at *e* refracted to *b*, (whence all other light is to be debarred,) which marked on the ruler (or, which I generally use as the better, on a table covered with white paper and perpendicular to *SQ*) gives the measure of the three sides of *Qeb*, whence we may command *cd*, which with *fg* = (s) *SlQ* affords all the variety produced by the change of inclinations; as if the plates be *wa*, *va*, the ray of incidence *ate*, refracted at *e* to *h*, thence to *i*, it will be *fg* : *cd* : : *po* : *mn* : : *qr* : *ts*, (which is the same with diag. 1, supposing *Ql* here, the tangent of the glass at *b* there); if *yx* be the ruler, *xi* the table, and the perpendicular to *l* fall between *Q*, *S*, the same proportion will be found in all the various inclinations. The glasses in the diagrams are distinguished by the red lines, and although I chiefly use for the angle-measuring glass, diagram 5, yet I intend herein to begin with the 1st, comprehended by the plane *aH*, and convex superficies *akH*, wherein *AB* a parallel ray (or according to truth not sensibly differing from it) is revolved upon

[c] *s*. 42° 15′ : *s*. 30° : : 250 : 186.

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the half wideness *iB* at *B* refracted unto *C*, the best appearing intersection of the refracted rays and the continued axis of the glass (or the centre of the sun's idol upon a table), by some exact scale I measure this distance from *k*, and hereby have all that is required in the precept appertaining to the 1st diagram, as will appear by the other included paper [d].

Although I have not hitherto glasses according to my own desire, yet they will as sufficiently as most other London best sale glasses serve for trials, in one whereof *Wk* is (as near as I can find) 2580, *ib* 60, iWB 1° 19′ 56″ = WBA, CBD by comparing of divers observations is near 2° 5′ 52″, whereof here is a proof agreeable to the other half of the diagram 1.

$10\frac{1}{2}$ *Sk*
4702 *iq*
43′ 52″ *iqb*
89° 16′ 8″ $ibq - ibg$ 1° 19′ 56″ $= gbq$ 87° 56′ 12″ $= 90 -$
qbr 2° 3′ 48″

100800 *ZS*, 2′ 4″ *omz*, here *mt* makes no sensible difference, being near 0.0037 of $\frac{1}{100}$ of an inch; hence it appears that *omz* 2′ 4″ + *qbr* 2° 3′ 48″ = 2° 5′ 52″ *CBD*.

Also, that *DE*, *GF* being any way exactly found, where the glass is of power to give *q* (differing from *C*) exquisitely, we find *Z* which hath not in this glass, at two miles distance, 1″ difference from 0″ 00″ &c., although Scheiner, p. 99, Refract. Cœl., thinks there is a difference proportionable to the perig. and apog. ☉.

[d] Viz. "in diag. 1. having $\left\{\begin{matrix} WB \\ BI \end{matrix}\right\} \begin{matrix} BWi = WBA \\ WBi \end{matrix}$
$Wi + ik = wk$
$wk + kC = \left\{\begin{matrix} iC \\ iB \end{matrix}\right\} iBC - iBg (= iWB) = gbi = 90 - CBD.$

Also $\left\{\begin{matrix} qi \\ ib \end{matrix}\right\} ibq - ibg = gbq = 90 - qbr$

Then $GF : DE :: (s.)\, mbW : (s.)\, rbq :: (s.)\, bmv\ (= tbm = tbW - mbW) : [s.]\, omZ.$

Surely the true cause of that summer and winter difference of the glasses' distance is from the rarefaction and condensation of the air, the change whereof is discerned by unchangeable glass. There is another reason, whereof hereafter.

In the first half diagram 2.[e]

$37\frac{1}{2}$ *Bm*
217 *iB* (as near as I can find)
6300 *kg*
$637\frac{1}{2}$ *Dg*

whence *HG* is to *EF* : : (s.) 15°.2449 : (s) 9°.8047, for which in the observations, hereafter recited, I use 15°.40 to 9°.95 having first found and used it: except I were more certain of all the other requisites I need not more curiosity: it may be, some seconds or decimals are something differing from the truth, because for ease I generally find the proportional part on a pasteboard double-rimmed circle of proportions. The other half of this diagram, though it shew the respect of *a* to *e*, yet because *gc* [diag. 3.] must be very

[e] On the inclosed paper before referred to in the letter there are the following notices. NB. The arcs are repeatedly written for their sines.

"Diag. 2. $\left\{\begin{array}{l} ib \\ Bm = Il \end{array}\right\} \begin{array}{l} miB = ABE \\ mBi \end{array}$

$mi + km = ki$

$\begin{array}{r} kg - km = mg = \\ gD - gC(=mB) = \end{array} \left\{\begin{array}{c} BC \\ CD \end{array}\right\} DBC + iBC\ (=90 - mBi) = iBD.$

Also $\left\{\begin{array}{l} lk \\ kg \\ ki \\ ge \\ hd \end{array}\right\} \begin{array}{l} cm \\ bl \\ tba \end{array}$ $\quad \begin{array}{r} kg - kh = hg = \\ ge - fg(=dh) = \end{array} \left\{\begin{array}{c} df \\ ef \end{array}\right\} def$

Then $df : ef :: no\ (=ig = kg - ki) : oe\ (+og = ni) = eg.$

$\left\{\begin{array}{r} ni \\ cni \\ ci \end{array}\right\} nci$

$nic = 180 - nci - cni \quad 90 : ci :: (s)\ cim : cm :: [s]\ icm :$

$= 90 - cim \quad im, ki - mi = mk, [mk] + kl = ml = vb.$

$GH : FE :: (s)\ nci : (s)\ qcb\ (-qcr = cim)\ rbc = \left\{\begin{array}{l} cbv \\ bv = lm \end{array}\right\}$

$(s)\ qcb : (s)\ nci :: (s)\ cbv : [s]\ tba\ (= bal)$

$cv + vm\ (= bl) = cm.$"

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short, or no distinguishing of *e*, though *a* be a great light, I therefore leave it for the diagram 4, where *K* is the same with this *a*.

In the third diagram *di* is the representation of the semidiameter *af*, the true angle whereof *egf* at a very short distance becomes altogether unperceivably differing from *acf*, which is apparent if we take the observation in the paper pertaining to the fifth diagram *ql* the same of *id* here, is 19.725 [f], *pf* or *fa* (1 yard) 3600, *kq* or *cd* 4533, *kf* or *ca* (11$\frac{1}{2}$ score) 828000, which gives *acf* 14′ 56″ and *egf* 14′ 56″, also *dci* 14′ 56″; for *DE* 2° 5′ 52″ : *GF* 1° 19′ 56″ (diag. 1) : : *dci* 14′ 56″ : *gcb* 10′ −. And *cgb* 89° 50′ + : *gcb* 10′ − : : *bc* 10$\frac{1}{2}$: *gb* 0.029 = *ea*. Hence is plain that the difference between *af*, *di* in a short distance may be neglected.

It is not unworthy of a note that *dc* of itself is in no length without its *di*, or that *i* is not so fixed but that *ci* may be prolonged. For if in the first observation [g] in the paper *cd* be 159 in. +, *Hb* 3, this *d* is only limited by the rays falling like *ABC* in the first diagram, which are both the colour-bringing and distance-declaring rays; if we lessen *Hb* to $\frac{1}{10}$ of an inch, *di* will be the same at *cd* (only more obscure), and if we remove *d* to 170 inches or +, *di* will be proportionable, and *i* very distinct, which may be more prolonged (for aught I see, to the length of Tycho's or Landgrave's greatest tube) without any mixture of imperfection from the sides of the narrow passage (which mixture all observers that way have noted); only, if the glass be spotted, each spot will be the vertex of a cone, the base whereof will be in *di*, wherein their appearance is more or less according to

[f] Centisimes of an inch. W. O.
[g] See these observations annexed at the end of the letter.

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the diminution or augmentation of *Hb;* whence the first light of this truth came to my notice, that conjoining this third and second diagram into the fourth diagram, where if *HK* have 2580 for its semidiameter *HB* be 60, which admits rays sufficient to make *ge* the sun's semidiameter, after it hath passed through the concave *lkc* of 217 semidiameter, clear and unspotted (from the glass *k*), it is easy to reserve the same glory of light or colour and yet augment *ge* (the sun's representative semidiameter received on a table), or any object received into the eye, reserving the same convex, only augmenting *HB* (provided that it should not exceed 2580; in this the hyperbola is preferable before any other figure) and proportionating a concave of a less axis, according to the necessity of the given proportion.

I believe this in remote objects to be of no contemptible use; though Monsieur des Cartes seem to say directly contrary in his De la Dioptrique, p. 129, Pour ce qui est de l'epaisseur de ce verre (*HK*) elle ne peut de rien profiter, ni aussi de rien nuire, sinon en tant que le verre n'est jamais si pur et si net, qu'il n'empeche toujours le passage de quelque peu plus de rayons que ne fait l'air. All experiments duly made will confirm this possibility of the increase of objects, and therefore disallow all our narrow cases as incapable of this help. In the note of observations, according to every diagram, the sun's semidiameter observed according to this is 16′ 11″. Here is at large every particular, that if I have overseen myself, you may know wherein I failed. I will not say that these are true to a second, though I hope to shew before I conclude that it may be easily and certainly performed. In this there are so many requisites that it is hard to come near the truth.

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Oct. 31.[h]	$5\frac{1}{2}$	*kl*
	217	*ki*
	3430	*kh*
	200	*hd*
	7591	$hg=df$
	600	*eg*
	4091	*Kl*
Therefore	10804	$ig=no$
	400	$ef=eg-gf=hd$
	$222\frac{1}{2}$	*il*

ge is the sun's representation on a table, and *hd* another on a cross[i], covered with paper, and the bars drawn with circles of 1, $1\frac{1}{2}$, 2 inch. rad., the lowest part of the cross is jointed, to separate it from the cursor on the ruler, when I please to view the whole sun at *g*. $df : ef :: 90 :$ tan. $edf\ 3^{\circ}.0136=90-86^{\circ}.9864\ def.;\ df : on :: ef : eo\ 569.3\ (+og=ni\ 30.7)=eg\ 600;\ ci\ (=ki) : in :: cni=def : nci\ 8^{\circ}.1213=180-cni-nic;\ 90 : ci :: 90-cin : cm\ 19.28 :: mci\ (=cin) : mi\ 216.1\ (+ml=bv\ 6.4)=il\ 221\frac{1}{2}$.

$15.40 : 9.95 :: nci : qcb\ 5.2740-bcr\ (=cbv)\ 0.1663=cim\ 5.1077$.

$90 :$ tan. $cbv : bv : cv\ 0.0185+bl\ 19.26=cm;\ Kl : bl :: 90 :$ tan. $0.2697=16'\ 11''$, *lKb* semid. ☉[s].

$$\text{Dec. 18.}\ \left.\begin{array}{lr} kh & 567 \\ hd & 51 \\ kg & 4307 \\ ge & 250 \\ Kl & 4097 \end{array}\right\}\ 16'\ 30''\ \text{semid.}\ \odot^{s}.$$

[h] Oct. 31, 1640. See Flamsteed's Historia Cœlestis, vol. i. p. 3, where the result of this observation is printed.

[i] Two lines are here drawn at right angles, with a dotted circle, of which the centre is at their point of intersection. This probably is intended to indicate the manner of making the observation.

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The same day according to diag. 3.

$$\left.\begin{array}{l} cd,\ 16048 \\ 2\ di,\ 154 - \&\ 153 + \end{array}\right\}\ 16'\ 26''\ \text{semid. } \odot^{s}\text{. or near it.}$$

$$\text{Dec. 24. diag. 3. } \left.\begin{array}{lr} cd & 36239 \\ di & 173 \end{array}\right\}\ 16'\ 25''\ \text{semid. } \odot^{s}\text{.}$$

$$\text{Jan. 20. } \left.\begin{array}{lr} kh & 2460 \\ hd & 150 \\ kg & 10946 \\ ge & 600 \\ kl & 4085 \end{array}\right\}\ 16'\ 24''\ \text{semid. } \odot^{s}\text{.}$$

It seems by these that the sun's greatest diameter is very near 33′, and, by this little, that it goes the whole eccentricity I will not be confident, until I have a greater certainty by the least diameter; although I have observations which persuade me that it keeps very near ½ eccentricity of Lansberg.

The least semid. 15′ 53″ + 32″ = 16′ 25″ the greatest.

Lansberg's least 16′ 47″ + 1′ 12″ = 17′ 59″ his greatest.

If I had not too long trusted Lansberg and Hortensius, I should have ere this been furnished with observations sufficient to have decided that controversy between Kepler and Hortensius. At this present I am not confident of any more than this, that the greatest is about 16′ 25″.

This of the cross which I have only of late used, (and which I cannot, for the largeness of the rest of the instrument, use either after 10^h at this time of the year, or at all in the same room in summer, to have *eg* 6.00 inches,) hath certainly shewed me, that there is required a greater distance between the two glasses, to make the best representation, at *hd* than at *ge:* although I yet perceive scarce a glimpse of the true reason, yet I see this will plainly follow, that there is here some cause of Scheiner's error (before

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mentioned), who, as it appears, by and with Malapert his acquaintance, endeavoured to keep the same diameter, which therefore must needs have in the apog. ⊙ a longer *hd*, or *ge*, than in ⊙ perig., and therefore a shorter *Kl*. It may be, the reason of this hath its original thus: In my great glass, used after diag. 3, I always find that the best resemblance of the moon's fixed or the sun's moveable spots is at least 3.00 inches before (or nearer the glass than) the plane, wherein the limb is most perfect. If this in its horizontally parallel diameters had not been unchangeable, I should have doubted its fountain had sprung from some unnoted power in the atmosphere. Sure I am, this hath more pestered my thoughts than any thing I have hitherto met with; and at this present I rest thus conceited, that the first ariseth from the intersection of the greatest number or quantity of the rays moving, as in diag. 1.; and the latter from the section of the last of these with the diameter-measuring rays, which hath a kind of proof by the former related effect of the contraction of *Hb*, the perceivableness of this ceasing by the contraction.

After the old manner, without a glass, we shall find that there is more show of colour in an obscured room with a larger hole, and more perfection of the diameter's apparition in a less. If it be hard to shew some probability of the reason of this in diag. 4, we shall find it readily in diag. 5, wherein is comprehended an incredible rarity, *q* here being the same of *C* in the 1st, *ql* of *di* in the 2nd; the glass *ab* is rightly placed when *lbm* can move rightly here as *Zmbq* in the 1st: and, as there, the more remote *Z* is from *k*, the nearer is *q* to this, so here if we place an object between *q*, *k*, the nearer its approach is to *k*, the nearer according to the same reason is its picture in *eg* to *x*, and con-

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trariwise of the contrary. This is that admirable secret, which, as all other things, appeared when it pleased the All Disposer, at whose direction a spider's line drawn in an opened case could first give me by its perfect apparition, when I was with two convexes trying experiments about the sun, the unexpected knowledge. Presently, placing my eye some little nearer *x* than *m*, I perceived that the apparition was not so punctual in the eye as on the table, although I had diminished *Zk* answerable to the augmentation by the eye (for a convex added to the convex *ab* makes *Zq* shorter, and therefore *Zk* shorter,) and in diag. 4 a less convex (I mean of greater radius) being adjoined to *kc*, according to its power to diminish the operation of the concave doth also abridge *Kl*, (by which abridgment in both glasses it is very possible to prove what convex glass is of equal power with the eye), and therefore resolved that if I tried *ab* like a spectacle, and placed a thread where that glass would best discern it, and then joining both glasses, and fitting their distance for any object, I should see this at any part that I did direct it to. And so I found it (if you please to try it, the convex of a perspective and a good convex spectacle glass will let you fully see it), which the next night's trial confirmed strangely accurate and ready for finding the altitude of any small star.

Upon trial I found by the moon that *qk* (the same distance of *kC* diag. 1) remained the same, though I tried sundry convexes like *ab*, and that the moon's picture here was in every respect answerable, for apparition of its diameter, to an object of equal rad. placed here. Thence I begun to contrive for some excellent scale, which might afford me in as small parts, as were possible, *ql*, believing that art could not afford such

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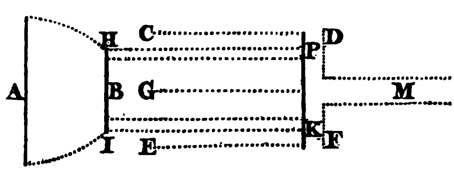
distinct divisions as might be severally distinguishable in *ql*. At last I contrived this many barred scale, one half whereof is here enclosed [k], whereof at first view you will readily know both [the] composition and application for use. The single bar at *g* is to be let into the case, where it is to determine *ql*, which if it be supposed to be the moon's rad. augmented by *ab* to the seeming length of a yard, or of ten yards, *pf* remaining the same, we shall find the same parts in the scale, what *ab* soever we use, hence by the reason of diag. 3. *kq*, *ql*, give $qkl = pkf$. I challenge no more in this scale save the decuplation, or, if need be, centuplation of the bars, and, therefore, thus ordered of their power. I hold a quadrant quaduated according to its right reason, and fitted for a mult-rimmed cursor, rightly applied to a hollow ruler, fitted with glasses and a hair, to be preferable before those that Morinus mentions (Scient. Long. p. 52, 3, 4, 5, 6), or any other at this present known to the world. I used in the sun's last eclipse one glass thus, and a little table to receive the sun's image, all on an iron ruler, moveable on the centre of a sextant of four feet, and found it very surely affording the sun's centre. After this I found by trial (what I desired in every part) as much ease as curiosity, and have here sent a catalogue of the moon's semidiameters observed by an imperfect scale, and sometimes not with a care of exactness. Such as they are, they prove the necessity of the bisection of the moon's eccentricity, as you will plainly perceive. Some other there are, which I only set here to shew what may be done. Indeed I defer the making of exact scales until I have got glasses wholly to my mind, having for this purpose caused a contrivance

[k] This drawing has not been met with; but a description of the scale will be found in the paper at the end of the letter.

for the certain forming of any requisite, either hyperbolic, or (according to Des Cartes) oval, convex superficies to be finished with some requisite tools or instruments for the drawing of the lines. I believe upon trial, Des Cartes's conceit will be found unuseful; surely, if I had not stayed only for an iron flawless plate, I had been furnished ere this with glasses of great command; but this country is very scarce of any skilful workmen in iron or brass, yet my desire not to be in any necessary defective hath caused me to try divers, and I think I shall shortly be fitted.

This glass in all astronomical uses is preferable before that of diag. 4, as 1st it may be contrived to admit into the eye at once 1, 2, 3° or more; the case and the glass *ab* being accordingly made in respect of the distance from *k*, whereas that only admits according to the proportion of the semidiameter of the perforation of the eye to *mc*, whence comes that mutation observed by Hortensius in viewing the moon at divers lights. Secondly, it may certainly measure this angle, so as Galileus's prodigious project in Syst. Mund. p. 381 and 2, will not be necessary. In the rude draught in the margin, suppose AB to be a plate of



brass of 20′ breadth in relation to *qk*, fastened to the two adjoining bars to the middle one G, and by a slit so let into the case as A B may be 10′ on either side *q*, and C G E concur with *nl* in the fixed part of the scale (whereof see the card). Then supposing the other half like this so placed as A of this concur with *b* of

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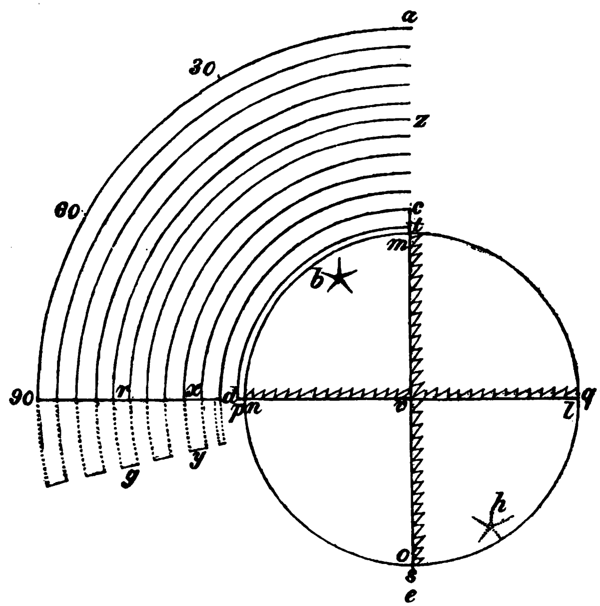
the other, and that CD, EF [i] may remove (as in the other card) by the help of the shank at M, A being near 40′, it will afford, when we please, without damage to our eyes, either the sun's diameter, according to the power of the scale and *qk*; or the moon's, which otherwise near the full in a wide *k* will offend the sight. AB being justly placed in *ql*, the two halves by the screws may be separated until both B, *b* concur with the outmost limits of the sun's diameter of longitude.

This is the easiest and most certain way that I can devise, and I tried a round plate of 154 in my greatest glass which did cover all the sun to the eye placed near M. I have attempted some trials by interposing green glass, which I find too troublesome, and therefore resolve, when I cause my cases to be perfected, to begin my observations, whereon I intend to rest, in that specified manner. If a hair be placed at right angles to the scale and pass through *q*, and be always moved in a vertical plane, in which setting it to one star, and the one of the brass pointers to another, and finding by both the pointers the distance between these stars, we have two sides, one of which is opposite to the right angle, and so a ready way to find the inclination, if this will content us. If not, here is an instrument may afford more exactness, though it be troublesome to fit into the case. Let *a*, 90, *dc*, be a quadrant graduated by transversal lines and many rims after the fashion of the fixed part of the scale, observing the true sites of each *zr* according to the allowance *ac*, 90, *d*, afford. Then fitting *rg*, *xy*, a moveable multrimmed brass plate, rightly graduated, and so let into *rx*, &c. as it make one

[i] The lines ought to have been continued on the figure from C and E to D and F.



plane with *a*, 90, *cd*, which is supposed on the back of the fixed part of the straight-lined scale, this cursor being fastened, by a brass pin *dp*, unto a concave cylinder *nmlo*, which is to be continued until it touch the brass pointers at *g* in the plane scale, at which end of the cylinder is to be placed a plate like *vnmlo*. One quadrant of the case *dptc* is to be so cut as the pin *dp* may move from *d* to *c* and carry with it *r* to *z*. Another slit, parallel to the axis of the case, must be made to let this pin into its true place, or the case must be there purposely jointed with a screw. If it be thus jointed it may be fitted to serve for the plates above, which otherwise must have their own length, wherein they are fastened, to themselves, which needs not to be above at most 1 f. long. When this is once placed, and *av* in a vertical position, it is ready for use. If *v*, *b*, be two stars, by any little pin in the cursor I

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move *rx* and its parallel *nv* until *nv* concur with the star *b*, the other being in *v*, therefore I have *evh* 30° the true inclination, because this glass shews all by contraries to the eye and right on a table. I had almost forgot that the graduations of the quadrant are to be continued from 90 to the line *yg*. If the outmost length of the case be made of wood, well squared toward the midst, and fitted to a standard perpendicular to the horizon, in which the upper part may be turned about, it will at all times be ready for use. It is also requisite to graduate that length of the case, that is next to the eye with a spiral line, divided by equidistants into what parts will best agree with the diameter-measuring scale for the ready finding of *qk*. It will be better if the case have two lengths than more.

The use of this is not comprehended within one degree, two, or three, but may by two observers and two dioptraes thus fitted with glasses, hairs, and moveable rims, be made serviceable for all angles, and give more than every second or decimal answerable unto them, which is much readier. Such scales were needless heretofore, when, by the confession of the best artists, there lurked at least 15″ or 30″ error even in the sun's best observations. I believe also by a ruler with a hair in it, moving upon the centre of a circular instrument graduated with transversal lines and two glasses, *ab*, *k* joined at a fit distance to the moveable ruler, and *k* placed in a perpendicular to the long ruler *km*, in a room not unfit for the purpose, we might in a few nights find the true meridian and pole, and, fixing the instrument there, have the star as a ready means any night to try the difference of ascensions between this and another, either planet or fixed. If the night be too dark to let us see the hair, a glimpse

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from a candle will help that, and not take away the use of the instrument. This I found in measuring the distances of stars in dark nights by the pointers.

If we allow that it is possible to have the moon in its greatest variation of altitudes, in its two limits in the meridian, to bear in any point of its visible hemisphere the same respect to the centre of the earth, it will follow that, from its own body, by this glass we may find its true parallax of altitude. August 1640. 25. I observed, diag. 5, ♃ 51″ diam. ♂ 38″. The glass not being potent enough to separate the two attendants of ♄ from its body, I could not measure it. Dec. 24. ♂ 25″, ♀ 25″ –, as near as I could discern their limbs by such ill-suited glasses as I yet am worth. Jan. 11th[k] the moon's lower limb was 30′ 38″ (the moon's diameter) above the northern eye of Taurus, the moon being very near the meridian, and its centre near 2′ southward of the star. Feb. 9, 1641, the moon's north-eastern limb and a star, præcedens duarum in colobio Orionis, (whereof I yet had no time to calculate the long. and lat.) were 15′ 2″ distant, the star vertical being some 4′ within the moon; they were 16′ 42″ separated at 36° 40′ alt. Cord. Leonis, the moon's eastern limb and the fixed above it in the same vertical; alt. Basilis. 39° 24′ dist. 19′ 2″; alt. Cord. 44°. 40′ dist. 30′ 54″ (= diameter of the moon when by a dial the moon shewed $1\frac{1}{2}$h. —). These altitudes were taken by a quadrant of no more than 1 foot radius; the elevation of the pole 53° 41′; our long. by the maps near that of Oxford.

Surely this glass will produce in a short time many exact observations for the finding of longitudes, if the moon's theory were within a few minutes true. It

[k] This and the following observations are inserted by Flamsteed in his Historia Cœlestis, vol. i. p. 3. In the first he reads 'occidentalior' in the place of 'southward.'

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will be of no small use for the correction of that and the observation of its eclipses, &c.; as also for the leaving perpetual testimonies of the other planets' situations to any known (or if we please as yet unnoted) fixed. But I know I need not, when once you have a glass thus fixed, to tell you a word of its use; and therefore, only adding two other as yet unnoted, I will conclude. The first is, that by two such as *ab* you may augment any small object placed near the one of them by the same reason of this, and if you please by the scale give the proportion of any part. If they be placed in a long case, and the object *Z*, diagr. 1, be so ordered as it may be light enough, it is an admirable representer as well as a wonderful augmenter of the species, which appear much more perfectly coloured than that of Des Cartes, p. 132, de la Dioptr.; only, as all other idols, they are pictured on the table covered with paper in their right site, and on the bottom of the eye representing or represented by that, which position, a man being in an erect station, is changed in respect of the horizontally equidistant station by the digression of the optic nerves after their conjunction, and by their reflection in their approach toward an embracement or mutual respect presenting themselves with their carriage into almost the midst of the great cells of the brain, where the ends have a contrary abode in regard of the vertical posture, as will appear to any that trieth it. The second and last is, that a third glass like *k* presenting an image in the place, where the last-mentioned small object was to be, it will appear to the eye right, and affords two planes for the scale, yet every apparition in greater obscurity than the other, which may be easily proved by the precedents.

If you judge these worthy of a public view, I shall

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labour to reduce them into the best order that I can, and to add some little more to them. I have shewed some part of these to two of my very late acquaintance, both industrious astronomers, who are not a little taken with them, and the more, because they believed they had tried all possible means, that glasses could afford for the measuring of diameters, yet never attained to any more than guessing, and indeed were so confident, as they would not believe until I let them see a glass to try the moon's diameter by, and how I proceeded in the other usual one. Indeed if one of my noble friends had not, before I intended it, bolted out so much as it could not longer lie hid, I had reserved it until I had got some rare glasses worthy of a prince's inspection, unto whose notice, at his majesty's being at York, he had, unknown to me, promoted this invention, who, as I conceive his words, will expect an ample relation. He also acquainted me with the famous Sir Kenelm Digby, unto whom I first shewed the contrivance of the scale, and its use in a glass, whose courtesy bound me to fulfil his request for some of the diagrams and observations, which I have lately sent him.

Good sir, do me the favour to let me know truly what you find upon trial, comparing these with what is already known, to which end I have largely and truly made relations of all these inclosed observations. What concerns engineering or aught else, every one according to their own employment will be ready to practise. That I lose no credit is the sole gain I expect; and that the lovers of art may know the advantage that this will afford, is the only end wherefore I would divulge it. If I add only a hair breadth to the knowledge of others, so it be useful, it will content me. If you please to let me know your mind, your letter at

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London may be delivered to Mr. Henry Gascoigne, at the sign of the Three White Lions, in the upper end of Paternoster Row, who will take care that it come safe to,

Worthy sir,
Your most obliged friend,
WILLIAM GASCOIGNE.

Middleton, between
Wakefield and Leeds.

OBSERVATIONS SENT WITH THE PRECEDING LETTER.

In diag. 3. $\left\{\begin{matrix} cd \\ id \end{matrix}\right\}$ *icd*

DE : *GF* (in diag. 1.) :: (s) *fge* = (s) *icd* : (s) *hgc* = (s) *bcg*. In this diag. $\left\{\begin{matrix} bcg \\ bc \end{matrix}\right\}$ $bg = ea + ef = af$.

By our large glass (suppose) *kcl*, Oct. 25, I observed $\left\{\begin{matrix} cd & 159.39 \\ di & 0.75 \end{matrix}\right\}$ (Inches) 16′ 11″ *dci* semid. ⊙s.

In diag. 4. $\left\{\begin{matrix} kl & 4091 \\ bl & 19.26 \end{matrix}\right\}$ 16′ 11″ *bkl* semid. ⊙s. Oct. 31.

I prefer this before the finding of *tba* by *cbv*, as less capable of error.

In diag. 5. $\left\{\begin{matrix} kq & 4362 \\ ql & 20.5 \end{matrix}\right\}$ 16′ 10″ *qkl*, Oct. 25, semid. ⊙s.

This observation was made by a green glass to obscure ⊙.[1]

Also diag. 5. $\left\{\begin{matrix} kq & 4533 \\ ql & 17.7\frac{1}{4} \end{matrix}\right\}$ 14′ 57″ *qkl*.

$\left.\begin{matrix} kf \text{ being } 11\frac{1}{4} \text{ score} \\ pf \quad 1 \text{ yard} \end{matrix}\right\}$ 14′ 56″ *fkp* the true angle.

This observation was made for the trial of the glass,

[1] These three are the observations, of which Flamsteed has inserted the results in the Hist. Cœl. vol. i. p. 3.

E 4

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as also for the correction of another glass whereby I had made most of the observations of the moon's diameters, whereof I found the convex next the eye was removed out of its due place by drying it, when it was in cold nights misted by the eye, which inconvenience may be avoided by laying either the whole, or that part next the eye, within the warmth of some fire before it be used. I first tried this glass in a glorious sun, wherein $2ql$ = two yards were precisely terminated by white paper, and kf $11\frac{1}{2}$ score yards carefully lined out; at which distance in the next dark night two candles were halved, hereby proving that the dilatation of the apple, or perforation of the eye, hath not that operation in this as in diag. 4, and therefore not liable to that error; whence ariseth Propri. 5. Hortens. p. 44. Dissert. de Mercurio in Sole.

$$\begin{array}{l}\text{In the same}\\ \text{book, p. 52,}\\ \text{he makes}\end{array}\left\{\begin{array}{l}\text{AC } 40'\\ \text{AE } 31\\ \text{EB } 18\\ \text{ aut } 20\\ \text{ED } 30\end{array}\right\}\begin{array}{c}\text{which}\\ \text{I find}\end{array}\left\{\begin{array}{l}\text{AC } 38'\,18''\text{ DF fere} =\\ \text{AE } 27\ 50\\ \text{EB } 19\ 24\\ \text{ED } 25\ 31\\ \text{AB } 35\ \ 1\\ \text{BD } 22\ 43\\ \text{AF } 22\ 54\\ \text{CB } 21\ \ 0\\ \text{DC \& AB fere} =^{\text{m}}.\end{array}\right.$$

I intend not here to set any thing but what I affirm is not so perfect as I resolve to rest upon. These by BD, ED, CD, will evince that for the present I am content with some error, yet I am sure ABED are very near the truth. I might also have placed many others adjoining to or contained in these, if I did not credit these sufficient.

[m] The measures of the Pleiades were made about August, 1640 See Flamsteed's Prolegomena, p. 95.

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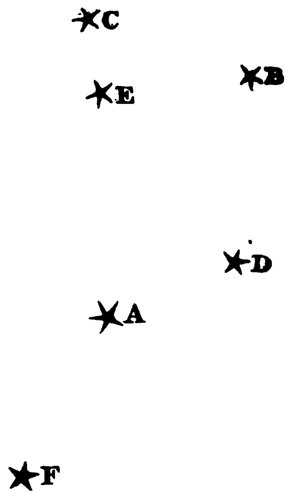

THE DESCRIPTION OF THE SCALE ON THE CARD, AND ITS USE.

The line *e* is 11 of *d* divided into 10. The other bars afford a decuplation of intersections performing as much as *d* and *e*, ten times their length, would do, by 101 of *d*=100 of *e*. So as if these [marks[n]] on the moveable *ge*, *sb* be brought in one straight line with their like to the fixed *nlgr*, we shall find 100+ on *d*, 40+on *e*; 4 on *f* for 144, the distance of *g* from the axis of the case. This moveable should be made of brass, and all the lines drawn as small as may be, and let into the fixed, that both have one common plane, if all be finished by a skilful workman. By a convex glass we may increase the scale, that we may discern + or − and make use of them, or if need be of more bars. A screw must be so contrived as it may remain parallel to *sb*, and being screwed through a staple near *ac* on the fixed, it may remove (by another standard near *s*, wherein it must be so fastened as it may turn about), and carry the moveable by its motion

[n] Representations of them are here roughly drawn in the original.

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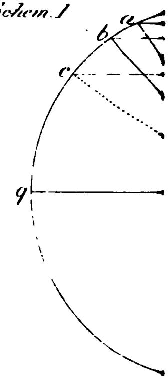
in and out. The other half must be ordered agreeably, and both *g g* meet at the axis of the case, and *d* continued.

CATALOGUS SEMID. LUNÆ.

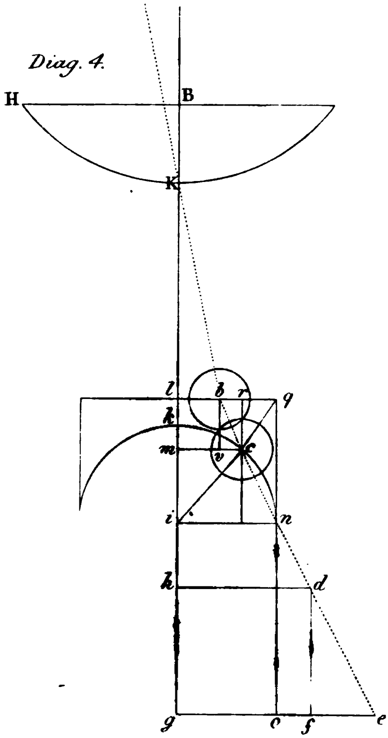
						h.
1640.	Aug.	25.	15′	17″		8 p. merid.
	Sept.	19.	15	11		8 p. m.
	Oct.	9.	16	36		8 p. m.
		10.	16	36		
		14.	15	54		
		15.	15	34		
		16.	15	28		8 p. m.
		17.	15	23		
		19.	15	2		
			15	1	diag. 3.	
		27.	15	38		
		29.	15	41		7 a. m.
		30.	15	43		
		31.	15	49		
	Nov.	9.	15	49		
		10.	15	40		
		14.	15	11		6 p. m.
		*16.	15	14		
		17.	15	21		
		20.	15	18		
		28.	16	45		7 a. m.
	Dec.	1.	16	18		
1641.	Jan.	11.	15	19		
		14.	15	41		
			15	23	diag. 3.	6 p. m.
		16.	16	0		
			15	40	diag. 3.	
	Feb.	9.	15	27		

* This 16 day I tried the moon by the glass, whereby I made the observation at 11½ score, and found that the diameter was 31″ less than by the glass I had made all the former observations by. Therefore it

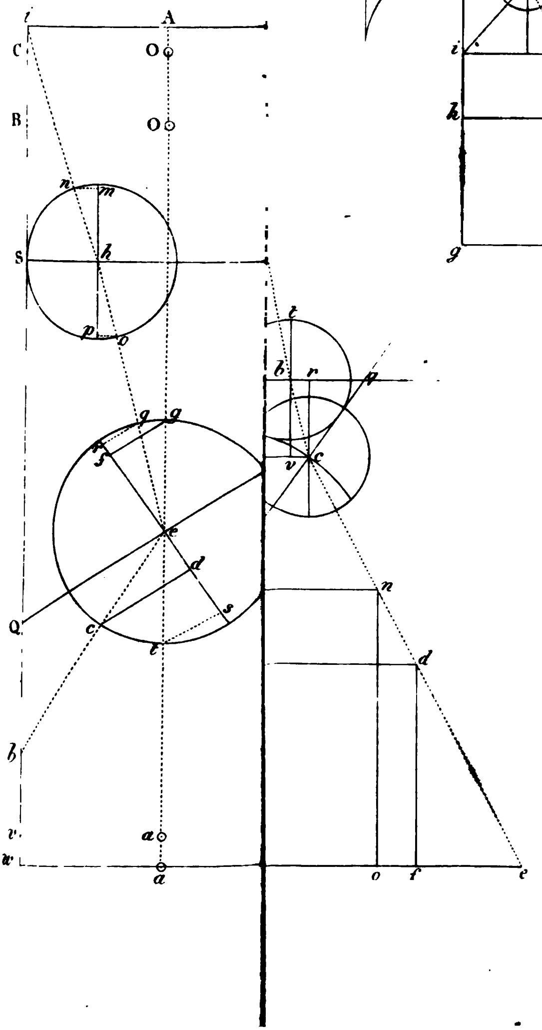
Schem. 1



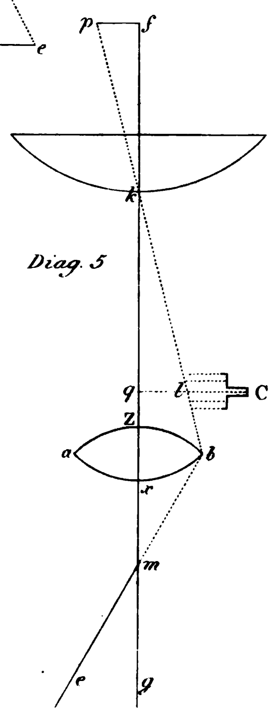
Diag. 4.



Schem. 2



Diag. 5



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seems they are to be 15″ less than are here set. I also found the distances of the Pleiades formerly observed agreeing hereto. And therefore made all these after by the tried glass.

Here are three more which the time will not permit me now to calculate:

Feb. 11.	21.1¼,	Rad. Lunæ *ql*,	4485 *qk*.
13.	21.5		4489.
16.	21.8		4488.

The last memorandum seems to indicate that the letter was written in Feb. 1641. A continuation of these measures of the moon's semidiameters may be seen in the Hist. Cœl. vol. 1. p. 5.

XXI.

PRICE TO OUGHTRED.

Sir,

Though I am a stranger to your person, yet I am well acquainted with the fame of your singular skill in the mathematics, and thereupon have so far presumed, as to intreat your assistance for the geometrical solution of the inclosed diagram, which, to you that have attained the perfection of the analytical art, perhaps will not appear difficult. But, whether all that may be performed by algebraical equations may likewise be wrought geometrically according to a lineary operation, I am not able to resolve, and therefore intreat to be instructed from you.

The present proposition will haply conduce to the invention of that so much desired and long sought for problem, for the finding of two mean proportionals between two extremes given, which having hitherto exercised the wit and endeavours of the most famous

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geometricians, I presume would not prove unworthy your pains, who, by the general suffrages of all men, are deservedly reported the ablest mathematician that our age knows.

Sir, I have been beholding to Mr. Elias Allen for the conveyance of this letter; and if you will vouchsafe me the favour, at your best leisure, to return me two or three lines in answer, and cause it to be left with Mr. Allen for me, I shall rest very thankful for the courtesy; and ever remain,

Yours to be commanded,
in what I am able,
WILL. PRICE.

London, 2nd of June,
1642.

This letter is printed in the General Dictionary, vol. viii. p. 83.

XXII.

OUGHTRED TO PRICE.

Sir,

It is true that I have bestowed such vacant time, as I could gain from the study of divinity, (which is my calling,) upon human knowledges, and, amongst other, upon the mathematics, wherein the little skill I have attained, being compared with others of my profession, who for the most part contenting themselves only with their own way, refuse to tread these salebrous and uneasy paths, may peradventure seem the more. But now being in years and mindful of mine end, and having paid dearly for my former delights both in my health and state, besides the prejudice of such, who not considering what incessant labour may produce, reckon so much wanting unto me in my proper calling, as they think I have acquired in other sciences; by

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which opinion (not of the vulgar only) I have suffered both disrespect, and also hinderance in some small performents I have aimed at. I have therefore now learned to spare myself, and am not willing to descend again in arenam, and to serve such ungrateful muses. Yet, sir, at your request I have perused your problem, the effect whereof is this: Datis positione tribus lineis in uno puncto concurrentibus, e signato in alterutra extremarum puncto lineam ducere sic ut segmentum inter duas reliquas interceptum, datam habeat longitudinem. Which problem, as also others concerning mesolabium (whereof I have studied and inquired many), I hold not feasible by any mere analytical way; but that they require some kind of mesographus, either line or instrument for that purpose. But you will say, that is mechanical and inartificial. And are not ruler and compasses, by which we yet solve all problems geometrically, also mechanical? And why may not we serve ourselves in locis solidis with their proper instruments, and yet do all artificially?

Datis AB, CD, et angulis ad B quæritur BC, vel BD vel AD.

Your problem is easily wrought per Nicomedis conchoidem lineam. If you draw in your scheme the pricked line AEF perpendicular to the middle line, and thereon measure EG equal to the length DC given; then from the point G trace the conchoides, and it will cut the line BC in the point C, which was desired.

But for your further favour and satisfaction, I have sought out a way analytical-like to find the angle BAC, by the rule of false position. I have not leisure to work it: but I make no doubt of the truth thereof. And though the operation be laborious, yet curiosity will accept of it.

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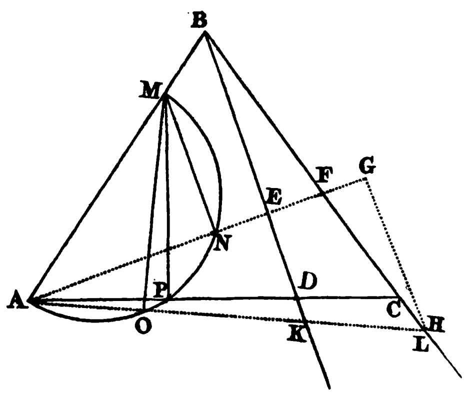
The way is thus. Out of the point G taken as was before shewed, in the perpendicular AEF, produce GH parallel to the middle line BD, and cutting the line BC in H; then draw the pricked line AKH, and upon it measure KL equal to the length given EG or DC. Here are now two positions. The first position is the sinus of the angle BAG; and the error of that position is FG deficient, in this example, wherein the point E falleth short of D: the second position is the sinus of the angle BAH, and the error of that position is HL abundant. But if the point E shall fall beyond D, both errors would be abundant. These things are very evident. Both these positions and errors are to be sought out by trigonometrical calculation, thus.



In triang. recto ABE	r : s. B : : AB : AE.
	r : cos. B : : AB : BE.
In triang. recto BEF	r : t. B : : BE : EF.
	EG − EF = FG. 1st error.
In triang. rect. FGH	r : cot. B : : FG : GH.
In triang. rect. AGH	AG : GH : : r : t. GAH.
	s.GAH : r : : GH : AH.
In triang. rect. AEK	cos. GAH : r : : AE : AK.

His sic inventis, erit s. (BAE + EAK) positio secunda

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in hoc exemplo. Et $AH - AK = KH$. Et $KH - EG = HL$. Error secundus.

And, having both positions with their several errors, with them work the rule of false position, which is this:

Multiplica positiones per alternos errores. Et, si errores sint ejusdem generis, differentiam productorum per differentiam errorum, sin diversi, summam productorum per summam errorum divide.

Effectio geometrica. Sumpta AM diametro pro lubitu, describatur semicirculus secans lineam AE in N, AH in O, et AC in P; ducanturque MN, MO et MP.

Tum fiet $X^2 = MN \times HL$ et $Z^2 = MO \times FG$. Sitque $X^2 \pm Z^2 = Y$.

Postremo dic $FG \pm HL : Y :: Y : T = MP$.

Junii 6°. 1642.

The beginning of this Answer to Price is printed in the Gen. Dict. vol. viii. p. 84; but the latter part, consisting of the enunciation of the problem, with its solution, and the diagram which illustrates it, are all omitted.

XXIII.

KEYLWAY TO OUGHTRED.

Sir,

By the small but favourable conversation you have been pleased, sometimes heretofore, to bestow upon me, I am encouraged to request as much more thereof, as your other occasions and studies, of greater moment, may (at least missively if not personally) permit. And for an entrance into such intercourse I make myself an object to your further courtesy, intreating, that your exchange of a paper may return me a relation of the

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most material uses, whereunto an exact quadrature of a circle is appliable, therewithal to refresh my late hard study and labour, to attain an ability to expose before the authority of your judgment an infallible demonstration thereof, when with convenience and privacy your desirable presence shall comfort and honour

Your friend,
in assured affection,
and in submissive reverence,
servant,
ROBERT KEYLWAY.

London, 8^{bris}. 26°.
1645.

This letter is printed in the Gen. Dict. vol. viii. p. 84.

XXIV.

OUGHTRED TO KEYLWAY.

Worthy Sir,

There was indeed left at my house a letter[o] divers days after the date thereof, but I neither knew by whom it was brought, neither was there any direction whither I should return my answer: besides, I have had shewed unto me divers overtures of such as have attempted that mystery of art, that have in trial failed. And we know that Orontius, and Joseph Scaliger, and Longomontanus, great masters in that science, have rather made demonstration of their heroical endeavour, than of the subject they proposed. And to say the truth, I have held the disquisition as infeasible, because, although no doubt there may be a right line

[o] There are two copies of Keylway's letter; and it would appear from what Oughtred says, that the second was sent in consequence of no answer being received to the first.

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equal to a circular, yet they being heterogeneous, there cannot in art be any due comparison between them, for comparatio is homogeneorum. We have out of Archimedes' grounds both by Van Ceulen and his follower Willebrord Snellius, so near an equality of a right line to a circle, that if the whole universe were sand, there would not be the difference of one small grain, and yet we cannot say the exact quadrature is by them delivered. Neither will it serve to those yet undiscovered uses which the proper and genuine equation (such as I conceive it must be) will no doubt afford, as namely, the mensuration of segments of circles, spheres, cylinders, and cones, with the frusta thereof; the solution of the problems, in the second book of Archimedes de sphæra et cylindro, which now are only soluble by cubical equations, the measuring of menisci and arbeli, and other figures, composed either of right lines and circles, or of different circles, all which would be most excellent discoveries and plus ultras in art. And I should also thence expect a facile and ready way for the proportion of subtensæ in a circle to their circumferences. These, and many other abstruse adyta, hitherto almost despaired of, may, I suppose, by your magisterial invention, tanquam Mercurii caduceo, be set open.

Sir, this is the effect of all I am able to answer to your desire, unless I more fully knew the manner of your way. I speak this the rather, and am induced to a better confidence of your performance, by reason of a geometric-analytical art or practice found out by one Cavalieri, an Italian, of which about three years since I received information by a letter from Paris, wherein was prælibated only a small taste thereof, yet so that I divine great enlargement of the bounds of the mathematical empire will ensue. I was then very desirous

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to see the author's own book while my spirits were more free and lightsome, but I could not get it in France. Since, being more stept into years, daunted and broken with the sufferings of these disastrous times, I must content myself to keep home, and not put out to any foreign discoveries.

Thus, with thankful acknowledgment of your so noble favour to design me worthy the communication of such a secret, I rest ready to do you all service which may be within the power of

Your humblest servant,
the true honourer of your worth,
W. O.

This letter is printed in the Gen. Dict. vol. viii. p. 84.

XXV.

DR. TWYSDEN TO OUGHTRED.

Worthy Mr. Oughtred,

The profit I formerly received by the first edition of your Clavis Mathematica, as also by your other works, made me not ignorant what might be expected from your pen: which, with the character I received from your and my noble friend sir Charles Cavendish, then at Paris, of your second edition of the same piece, made me at my return into England speedily to get, and diligently peruse the same. Neither truly did I find my expectation deceived; having with admiration often considered how it was possible (even in the hardest things of geometry) to deliver so much matter in so few words, yet with such demonstrative clearness and perspicuity: and hath often put me in mind of learned Mersennus his judgment (since dead) of it, that there was more matter comprehended in that little book than in Diophantus, and all the ancients.

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Sir, as I hold it below an ingenuous spirit to flatter where worth is not, so do I think it little less than robbery to keep back from you the knowledge of that estimation [which] is put upon your person and book in foreign parts; which I wish heartily might serve as an encouragement to set you forward in the publication of other things of the like nature, of which (as I hear) you are not unfurnished; that putting together all done by you, both in the Latin and English tongue, the two most famous sciences of geometry and astronomy, both for solid precept and instrumental practice, might receive (for the honour of our nation) from yours more advancement than from any one hand whatsoever. And amongst instruments (which are good helps but ill masters), why might you not be persuaded to publish the description of that mentioned by you elsewhere, for the ready supputation of the places of the planets, as a backside to your double Horizontal? Which though particular, might in its chiefest use of finding the meridian line be made (as I think) general, by fixing it on a moveable foot, and then being elevated or depressed according to the different elevation of the pole, and turned about at the same angle till the double shadow point out the same hour, standing thus in the meridian, be made use of for the protraction of the hour lines upon any plane, without respect had to its situation, by a thread passing from the centre of the horizontal dial through the intersection of the hour spaces continued to any plane whatsoever.

But to return whence I have digressed. Amongst many other excellent rules in your Clavis, I cast my eye upon that example of the first rule of doubling, &c., the angle of a rectangle triangle opposite to the base, but must confess could never perform by it what

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I sought, as by the second I ever did. The result of my work, according to both your rules, you will find hereto annexed, in which I am bold to beg your judgment where my fault may be, or a farther explication of the same, being assured of your civility and goodness, and glad also to snatch any occasion to renew that little acquaintance I formerly have had of you by our casual meeting at Mr. Allen's in the Strand, by whose means I send this, and who will do me the favour to return your answer hereunto, if you shall please to send it him, directed to sir Roger Twysden's house at East Peckham in Kent, for me that am,

Sir,

Your most affectionate friend,

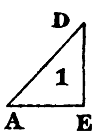
to serve you,

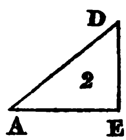
JOHN TWYSDEN.

East Peckham,
Feb. 18. $16\frac{49}{50}$.
Brought to me Feb. 24☉.

Triangula angulorum simplorum.

$AD = \sqrt{18}$
$AE = 3$
$DE = 3$



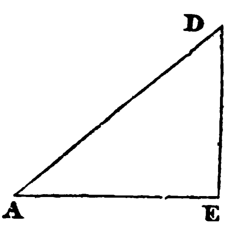


$AD = 5$
$AE = 4$
$DE = 3$
$DAE = 36^{\circ}\, 53'$

Triangula angulorum duplorum.

Juxta primam regulam.

$AD = 27$
$AE = 9$
$DE = \sqrt{648}$

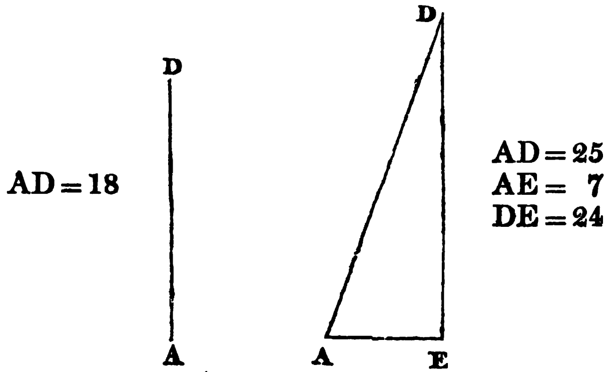


$AD = 34$
$AE = 16$
$DE = 30$

Sed his quantitatibus anguli non duplicabuntur (ut opinor); nam in exemplo numeri primi, angulus duplicatus debuit esse 90° 00′, quia triangulum anguli simpli est æquicrurum.

In exemplo secundo ut 16 : 30 : : rad. : tang. 61° 55′ circiter; differentia 11° 51′ min.; nam debuit esse 73° 46′.

Juxta secundum exemplum.



In triangulo æquicruro primo, hypotenusa trianguli anguli dupli erit 18, cathetus 18, basis 0, quod indicio est hypotenusam desinere in cathetum, vel esse ipsi parallelam, et propterea angulum duplicatum.

In triangulo secundo 7 : 24 : : rad. : tang. 73° 45′ dupli prioris, ut debuit esse, nam differentia unius scrupuli primi tribuenda est operationi per canonem sinuum &c. Sed in utraque regula angulus acutus trianguli simpli semper intelligitur opponi catheto non basi, uti mihi videtur.

(Upon this letter Oughtred has written the following remarks.)

Sumatur A 25, E 24, $A.E$ 600.

A^2 625, E^2 576.

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$$\begin{array}{c}\text{Erit. in trig}^{o}. \\ \text{ang. simpli}\end{array}\left\{\begin{array}{ll} A^2+E^2 & 1201\ H \\ A^2-E^2 & \quad 49\ C \\ 2A.E & 1200\ B\end{array}\right.$$

$$\text{Tum}\ \left.\begin{array}{lr} H^2 & 1442401 \\ C^2 & 2401 \\ H.C & 58849 \\ B^2 & 1440000\end{array}\right\}\text{unde}$$

$$\begin{array}{c}\text{Erit. in trig}^{o}. \\ \text{ang. dupli}\end{array}\left\{\begin{array}{ll} H^2+C^2 & 1444802\ h \\ 2H.C & 117698\ c \\ B^2 & 1440000\ b\end{array}\right.$$

Et sic ulterius erit progrediendum. Item in exemplis Reg. II.

Denique pro inventione angulorum subtensorum a cathetis.

Dic 1°. 1201 : 49 : : 100000 : 4080 = s. 2° 20′.
Dic 2°. 1444802 : 117698 : : 100000 : 8146 = s. 4 40.

This letter is printed in the Gen. Dict. vol. viii. p. 85; but all the particulars which Twysden added as a postscript, and Oughtred's remarks on them, are there omitted.

In the second edition of the Clavis, which was published in 1648, the following passage occurs at the close of the 16th chapter: it is not found in the first edition of 1631 (which circumstance agrees well with what Twysden says in the beginning of his letter). "13. Datis binis triangulis rectangulis, H, B, C, et h, b, c, tertium ex ipsis fabricare: idque dupliciter." H, h, are used to denote the hypotenuses; B, b, the bases; and C, c, the perpendiculars (catheti), the triangle being supposed to be so placed as to have the right angle at one extremity of the base. It is then shewn that by compounding these quantities, another right-angled triangle will be formed, in which

$$\left.\begin{array}{l} H'=H.h+C.c \\ B'=\quad B.b \\ C'=H.c+h.C\end{array}\right\}\begin{array}{c}\text{by the 1st} \\ \text{method.}\end{array}\ \left\{\begin{array}{c} Hh \\ B.b-C.c \\ B.c+C.b\end{array}\right\}\text{by the 2nd.}$$

It is then added, "14. Si trianguli rectanguli latera continue multiplicentur juxta binas regulas modo inventas, prima multiplicatio angulum basi oppositum duplicabit, secunda triplicabit, tertia quadruplicabit et sic ulterius." For this purpose H, B, C',

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are taken respectively equal to h, b, c, and then for the duplication,

$$\left.\begin{array}{l} H' = H^2 + C^2 \\ B' = \quad B^2 \\ C' = \quad 2H.C \end{array}\right\} \text{by the 1st method.} \quad \left\{\begin{array}{c} H^2 \\ B^2 - C^2 \\ 2B.C \end{array}\right\} \text{by the 2nd.}$$

Now these are the formulæ, by which Twysden worked to find a new right-angled triangle, in which an angle should be double of one in that, which he first took for his example: and he is right in all the conclusions[p] which he draws from the investigation. The complete failure of the first method shews that it cannot be used to answer the purpose, for which it was intended; and if he had examined the fundamental value of the terms in it, he would have found that it was inapplicable to his purpose. Oughtred has indeed, in his remarks, produced an instance in which it appears to succeed, but this is really fallacious. By reducing one of the sides to a very small proportion, when compared with the others, he has been led apparently to the desired conclusion, which however he would have found not to be accurate, if he had strictly worked out his own results. The quantities would then have given him the sines of 2° 20′ 17″.8 and 4° 40′ 21″.6.

The error respecting the situation of the angle, which was doubled, was easy to be set right; and if he had trusted to his second method, he would have found all come out accurately by it; but Twysden's objections, or others which may have occurred to himself, seem to have induced him to abandon the use of this formula. In the third edition of the Clavis (1652), he no longer applies the rules as before; but merely says, "Prima multiplicatio triangulum producet bicompositum, secunda tricompositum, tertia quadricompositum et sic ulterius." Now in the formulæ which are given as examples of the second rule, he really had obtained expressions, which gave him the sines and cosines of multiple arcs, as may be seen by merely substituting in them unity for H, cosine A for

[p] It may be remarked, that he would have seen this truth the more clearly, if he had been more minute in his calculations: the angles, which he calls 36° 53′ and 73° 45′, are really 36° 52′ 11″.6 and 73° 44′ 23″.2, the one precisely the double of the other.

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B, and sine A for C. (Possibly Oughtred considered that all was required for his purpose might be derived from the rule which in his first edition he inserted for this purpose at the end of his xviiith chapter. This there will be occasion to refer to more particularly hereafter.)

For the triangulum bi-compositum he shews that

$H' = H^2$ or 1.

$B' = B^2 - C^2$ or $\cos.^2 A - \sin.^2 A = \cos. 2A$.

$C' = 2B.C$ or $2 \sin. A. \cos. A = \sin. 2A$.

For the triangulum tri-compositum

$H'' = H^3$ or 1.

$B'' = B^3 - 3B.C^2$ or $\cos.^3 A - 3 \cos. A. \sin.^2 A = 4 \cos.^3 A - 3 \cos. A = \cos. 3A$.

$C'' = 3B^2.C - C^3$ or $3 \cos.^2 A. \sin. A - \sin.^3 A = 3 \sin. A - 4 \sin.^3 A = \sin. 3A$.

For the triangulum quadri-compositum

$H''' = H^4$ or 1.

$B''' = B^4 - 6B^2C^2 + C^4$. or $\cos.^4 A - 6 \cos.^2 A. \sin.^2 A + \sin.^4 A = 8 \cos.^4 A - 8 \cos.^2 A + 1 = \cos. 4A$.

$C''' = 4B^3.C - 4B.C^3$, or $4 \cos.^3 A. \sin. A - 4 \cos. A. \sin.^3 A = (1 - \sin.^2 A)^{\frac{1}{2}} (4 \sin. A - 8 \sin.^3 A) = \sin. 4A$.

XXVI.

AUSTIN TO OUGHTRED.

Most Rev. Sir,

I am ashamed that I am so slow in returning my due thanks to you for all the pains and learning, that you have so liberally bestowed upon me; and I am afraid you can not but have thought, afore this, that I am very ungrateful, and unworthy of the many favours, which I have received at your hands. But, sir, this is some comfort to me, that you know upon what occasion I was so suddenly called away from you; and truly the care and trouble which I had for my brother, whilst I was at London, did so fill and

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take up my mind, that I had little time or leisure to think of any thing else: and so I hope that may partly excuse me for that I have been so long deficient in my duty.

Sir, I acknowledge my obligation is so great, that it is not a letter will discharge me: whilst I live I must be your debtor; for I cannot, sir, but reckon you amongst the chiefest and noblest of my benefactors, seeing you have given me that, which is invaluable for its worth, and which no violence can rob me of, unless I please, and which I know nobody else but yourself could have given. I am now at Cambridge, but methinks, I do but lose my time here, when I compare my gains here with those at Albury. Sir, were I a free man to go whither I would, and could I be maintained whither I would go, I should quickly be determined upon my journey; for I know no place in the world, that I would choose rather to go to, than that where you are. Happy are those who always enjoy your company; happy are they who hear your wisdom and learning. I will not weary you, sir, with tedious expressions; I will only add this, that I truly honour and admire you, and that I shall always count it my duty to make such honourable mention of you, upon all occasions, as may beget in others the like esteem of your inherent worth and excellencies. Sir, in what I have said I am most sincere and real: and so presenting my most humble thanks and service to yourself and Mrs. Oughtred, I rest,

Your most obliged servant,

Coll. Reg. Dec. 3. 1652. ROB. AUSTIN.

Sir, I have sent you two little books (Harvey and Gassendus), which I think you have not yet seen, as a

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small recompense of the incomparable book you bestowed upon me, namely, your Euclid, which I shall always reserve by me as a great rarity, it being demonstrated by yourself after your own way, given and written by your own hand. I have desired my friend at London to buy me two barrels of olives and capers, which I desire you to receive as a poor token of my thankfulness. I hope I shall shortly procure Gassendus' Astronomy to send you; but they are very scarce, and hard to be gotten with us. I have not yet had leisure to review my papers, and so have no doubts to be resolved, but I shall make bold, sir, hereafter, when they occur, to acquaint you with them, seeing you have given me leave. I humbly desire you, sir, at your leisure, to note out for me, in short, the triangles, either in your Analemma or perspective, which serve for the last propositions in your astronomical operations, beginning at the 40th. I could wish too, sir, were it not too much trouble, that I had the demonstration of two propositions, which you were about to demonstrate to me, but never did them, viz. the last of the Elementa Sphærica—Si duo circuli maximi parallelorum minimum contingant, parallelorum segmenta erunt similia; and the other is, Si duo semicirculi super eandem rectam lineam sic statuantur ut centrorum ab initio lineæ distantiæ semidiametris ipsorum sint proportionales, recta ab initio lineæ protracta utrumque vel tanget vel secabit similiter.

I desire to be remembered to Mrs. Margett and Judith, and to all your sons. I hope Mr. Benjamin has his desires; his brother at London told me he did not doubt of it. I went to inquire after Mr. Porter, but he was not in London. I desire, sir, I may know whether he be with you or no.

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XXVII.

SETH WARD TO OUGHTRED.

Sir,

Being last week at London I called on Mr. Gratorex, who shewed me a letter which he had received from you concerning the late comet, wherein you desired that he would communicate your observations to such as he should meet with, and desire them to do the like. I took the boldness, therefore, to transcribe that part of your letter which concerned it, and I have here inclosed sent you such observations as were in my absence (for I was then in a journey) made here by Mr. Rooke, and one who belongs to me. If you shall be pleased to communicate to me what you observed to the last appearance, you will very much oblige me: and I beseech you, sir, to accept my most hearty service, and present it to Mrs. Oughtred and your children, being assured there is not any thing more considerable to me than to manifest myself upon occasion,

Your most humbly
devoted servant,
SETH WARD.

Mr. Rooke presents his humble service to you.
Oxon. Jan. 4. 1652–3.

1652.		hor.		long.			lat.		
Decemb. 11.	♄	11	pomer.	2°	45′	♊	18°	20′	A
14.	♂	11	——	27	30	♉	4	55	B
15.	☿	10	——	25	30	—	9	10	—
16.	♃	10	——	23	45	—	14	10	—
18.	♄	9½	——	22	50	—	19	0	—
19.	☉	9	——	21	40	—	21	30	—
21.	♂	9½	——	20	40	—	25	30	—
22.	☿	8	——	20	25	—	26	45	—
23.	♃	10	——	20	10	—	28	10	—
25.	♄	10	——	19	30	—	30	35	—
30.	♃	...	antem.	18	50	—	33	30	—[q]

[q] See Ward de Cometis, p. 39.

XXVIII.

AUSTIN TO OUGHTRED.

Rev. and most honoured Sir,

It is now very long since I last wrote unto you, but it hath not been the forgetfulness of my obligations that in the least hath been the cause of my so long intermitting the acknowledgment of your very great favours, but only, sir, a continual expectation to hear a word or two from yourself, first, of the receipt of what I last sent you. And I begin to mistrust that it was through the negligence of the post miscarried. I sent you, sir, two books, one of which was a good large quarto of the Greek Musicians set forth by Meibomius, the other an Antidote against Atheism writ by one Mr. Moore of our university, whom I spoke to you of. I should be sorry, sir, to hear, what I expect, that they never came to your hands.

I was lately, sir, at London, upon a sad occasion, called up to see a dying friend, Mr. Wheelock, Arabic Professor to our university; and I saw him buried before I came away. As soon as I had any leisure I went to your son's house to inquire after your welfare, where I met with Mr. John, who told me of the great fit of sickness you have lately had. I give God humble and hearty thanks for your recovery, and I beseech him to prolong your days for the public good, though I know, sir, how little you care for this present life.

Sir, I have received wonderful benefit from the pains you bestowed upon me; a great while I had nothing but a confused knowledge; but now I see things clearly, aperuisti oculos, jam lucem video. There are very few things I stick at, yet some there

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are. I crave leave, sir, to propound three or four to you [q].

1. Concerning the fourth case in multiplication; the rule is, si velis factum multatum locis aliquot integrorum puta quinque, &c.: in the example methinks the figures cut off, all save one are parts, and those which you put for parts 0.0027 are integers according to the indices.

2. In the three species of reading in ordine scalæ, I suppose it is because their indices ascend that you call them so; if they be 0, 1, 2, or 0, 2, 4, why must, sir, the index of your Æ be 0? Methinks it should at least be as high as ZA, or in the next $Æ^2$, as ZA^2.

3. Pag. 62, line 25. Si numerus negatorum laterum sit impar, species illa erit negata. I cannot find, sir, what you mean by negata. I see there are some particular products negatives, but so there would be if numerus laterum negatorum esset par. Then let all three be negative, numerus esset impar, and yet all would be affirmative in multiplication.

4. I cannot understand how $4R^2 \times OA^2 - OA^4 = R^2 \times OB^2$ is the bisection of an angle; for if the radius be 1, which alters nothing in multiplication or division, then how does bisection differ from duplication, or how does $4R^2 \times OA^2 - OA^2 = R^2 \times OB^2$ differ from $\frac{4R^2 \times OA^2 - OA^4}{R^2} = OB^2$, the radius and all its potestates being to be omitted, or making nothing?

This is all, sir, I scruple at in your Clavis; and could I resolve these, I believe I could give any one

[q] These all refer to the third edition of the Clavis, which was published in 1652. The subject of the 1st will be found at p. 10, of the 2nd at p. 52, of the 3rd (as above specified) at p. 62, and of the 4th at p. 108.

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good satisfaction in any proposition throughout the whole book. Thus, sir, with the acknowledgment that I have received more from you than ever I shall be able to requite, and the presenting of my most humble service both to yourself and Mrs. Oughtred, I rest,

Sir, your most humble servant,
to my utmost power,
ROB. AUSTIN.

Coll. Reg.
Nov. 1, 1653.

If you please, sir, to write to me, you may direct them to be left at the sign of the Greyhound in Little East Cheap with my brother John Austin.

On the back of this letter Oughtred has written as follows:

1. Pro quæsito primo de casu quarto in pag. 10. Nil aliud atque computatio hujus proportionis.

$$
\begin{array}{rl}
100000 : 42262 : : 0.0064 & \\
\underline{0.0064} & \\
169048 & \\
\underline{253572} & \\
0.002704768 & \text{factus totus.}
\end{array}
$$

Ex toto facto abscindendæ sunt linea separatrice novem ultimæ figuræ, nempe quatuor pro partibus decimalibus minoris et quinque pro circulis, per pag. 7, lin. 10, 11, 12, et per pag. 12. sect. 3.

Sunt autem tantummodo figuræ septem; quare duo loci partium supplendi sunt.

$$
\begin{array}{r}
\text{Vel sic; } 1.00000 : 0.42262 : : 0.0064 \\
\underline{4600.0} \\
25 \\
\underline{2} \\
27
\end{array}
$$

Quia quatuor loci partium requiruntur, statuatur

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unitatis locus minoris 0 sub 6 in quarto loco partium majoris.

2. Pag. 37. Ordo scalæ in tabella posteriore, sic est

	Absolut.	Latus,	Quad.	Cub.	Q. Quad.	Q. Cub.
nempe	0	A	A^2	A^3	A^4	A^5

In equatione $Z.A - A^2 = Æ$, vel $A^2 - XA = Æ$, &c. rectangulum Æ non est gradus adscensionis (hi non sunt A et A^2, &c.) sed absolutum, sive principium unde ascensio fit: estque numerus resolvendus in suos factores sive radices A et E. dantur quidem Z, X, Æ; sed A et E investigandæ sunt.

3. Pag. 62. De $C : A + E - I$. Dico I^3 est negativum, nempe $-I^3$. Nam $-I$ in $-I = I^2$ affirmativo quia signa sunt similia, tum hoc I^2 in $-I = -I^3$, quia signa sunt diversa. Item A^2 in $-I = -A^2I$, quia signa sunt diversa.

4. Pag. 108. Verum est quod æquatio $4OA^2 - OA^4 = OB^2$ (omisso radio 1. cum omnibus potestatibus suis) inservit tum duplicationi, tum bisectioni: sed modo multum diverso. Nam in duplicatione, ex dato OA simplo, facillime per multiplicationem invenitur, multuplum OB. Sed ex dato OB multupla investigare OA simplum, arctus est labor, cui sequens tractatus De Æquationum adfectarum resolutione in numeris addicatus est.

XXIX.

WALLIS TO OUGHTRED.

Sir,

I have herewith sent you a trifle of my own (an observation of a solar eclipse of last year[r]) not worthy

[r] See Wallisii Opera, vol. i. p. 483.

your acceptance, yet such as the respect, which I owe you, commands me to present unto you, having nothing better to tender, and to assure you that I am, sir,

Yours in all observance,

Oxford, Feb. 5.
1654–5.

JOHN WALLIS.

Sir,

If your occasions may permit you, I should most earnestly desire your help in the solution of the problem ensuing, which, if not from yourself, I know not whence to expect. The use which I shall be able to make of it will be very well worth some pains, which makes me the more bold to trouble you with it, and crave your pardon for that presumption.

Expositis quantitatibus $A \quad B \quad C \quad D$ &c.
Quarum rectangula sunt $AB=1 \;\; BC=2 \;\; CD=3$ &c.
Quæritur, quanta sit $A \;\; B \;\; C \;\; D$ &c.

Annexed to this letter is the following solution of the question in Oughtred's handwriting.

Esto A numerus aliquis, sive integer, sive fractus: puta 3.

$\genfrac{}{}{0pt}{}{3}{1}\Big)\genfrac{}{}{0pt}{}{1}{1}\Big(\frac{1}{3}$	$\frac{1}{3}\Big)\genfrac{}{}{0pt}{}{2}{1}\Big(\genfrac{}{}{0pt}{}{6}{1}$	$\genfrac{}{}{0pt}{}{6}{1}\Big)\genfrac{}{}{0pt}{}{3}{1}\Big(\frac{1}{2}$	$\frac{1}{2}\Big)\genfrac{}{}{0pt}{}{4}{1}\Big(\genfrac{}{}{0pt}{}{8}{1}$	$\genfrac{}{}{0pt}{}{8}{1}\Big)\genfrac{}{}{0pt}{}{5}{1}\Big(\frac{5}{8}$	$\frac{5}{8}\Big)\genfrac{}{}{0pt}{}{6}{1}\Big(\frac{48}{5}$	
A $\times$ B	B $\times$ C	C $\times$ D	D $\times$ E	E $\times$ F	F $\times$ G	&c.
1	2	3	4	5	6	

Rectangula sunt 1, 2, 3, 4, 5, &c. Dividatur unumquodque per unum ex lateribus suis, ut habeatur latus alterum. Latera autem sunt A, B, C, D, &c.

This letter, with Oughtred's note, is printed in the Gen. Dict. vol. viii. p. 86.

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XXX.

STOKES TO OUGHTRED.

Honoured Sir,

Since I left you, I have recollected, as much as my journeys before, and necessary exercises after my return to Cambridge would permit me, those notions I received from you. The first thing I did was to follow your counsel in perfecting myself in the projection and trigonometry, which I think, sir, I do now fully understand. Some other things also I have reviewed; but find more occasion to acknowledge my engagements to you for your clear instructions than to trouble you with many doubts. Yet some things there are, sir, which, presuming upon your goodness and the encouragement you gave me, I shall desire to be satisfied in.

1. As Clavis, p. 100. Quare Theorema primum de mensurando frusto Coni sit $\frac{355}{452}A^2L+\frac{355}{452}A^2T-\frac{355}{452}E^2T=$ triplo frusto. I do not understand, sir, why you say $\frac{355}{452}$ not $\frac{355}{113}$, when I suppose you mean the area multiplied into the height D, as before 'tis, say $\frac{355}{113}R^2=$ area circuli. Pray, sir, shew me what I have misapprehended, and wherein my error lies[s].

2. In p. 1. Archim. de Sph. et Cyl. §. II. $\frac{\pi}{\delta}M^2$ is proved = superficiei curvæ coni, because it is $=\frac{\pi}{\delta}$ AO in

[s] Quia $D^2=4R^2$. sunt autem A et diametri, non radii. [W.O.]

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KO; but that the semiperiphery into the side of a cone is = the curved superficies, I know not how to demonstrate.

3. Another thing which troubles me is in p. 37, de Sol. Reg. where the superficies (4) is 4.618628, the solidity but 0.513216, which seems less than the superficies. I must humbly crave your assistance, sir, to be in these; and if I trespass not too much upon you,

4. How, in Clav. p. 108, the duplication of an angle differs from the bisection; since the radius alters nothing.

Sir, I have procured your Trigonometry to be written over in a fair hand, which when finished I will send to you, to know if it be according to your mind; for I intend (since you were pleased to give your assent) to endeavour to print it with Mr. Briggs his Tables[t], and so soon as I can get the Prutenic Tables I will turn those of the sun and moon, and send them to you. Sir, my acknowledgment of what I owe to you is all I shall further add; only I must desire your acceptance of an Hierocles on Pythagoras (which I have sent with this letter), from him, who shall always, as his duty binds, pray for you, and remain,

Sir, your obliged honourer,

RICHARD STOKES.

Coll. Reg. Cantabr.
Feb. 6th, 1654–5.

Sir,

I, as you commanded, carried your commendations to our Provost, who desires his may be returned to you.—Sir, you may send your letters to me to be left with Mr. Grettricks.

[t] Oughtred's Trigonometry was published in English and in Latin by Rich. Stokes in 1657.

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XXXI.

OUGHTRED TO STOKES.

Your first quæsite is, why (in pag. 100, de mensurando frusto coni) I say not $\frac{355}{113} R^2$, but $\frac{355}{452} D^2$? The reason is, because in the scheme the lines A and E (sides of the quadrate bases) are the diameters of inscribed circles, which are the conic bases, and not radii or semidiameters. Now because $D^2 = 4R^2$, the area of the circle shall be $\frac{355}{113} \times \frac{D^2}{4,}$ that is, $\frac{355}{452} D^2$.

Your second quæsite is, why (in pag. 1. Archim. de Sph. et Cyl.) semiperipheria basis coni in latus æquetur superficiei conicæ, that is $\frac{\pi}{\delta}$ AO × KO = the curved superficies of the cone? The reason is evident. Because the superficies [of] an equilateral cone is a sector of a circle, inflected round, the centre being the vertex. But the area of the whole circle is equal to the half ambite multiplied by the radius (nempe $\frac{\pi}{\delta} R$ in R). Wherefore the area of a sector shall be equal to half the arch cut out multiplied by the radius.

Your third quæsite is, why (in pag. 37 de Solid. Reg.) the number of the superficial measure is still greater than the number of the solid content? The reason is most perspicuous. Because the one is reckoned in unciis quadratis, the other in cubicis.

Your fourth quæsite is, why (in anguli tum duplicatione tum subduplicatione, pag. 108) the equation whereby it is solved is the very same in both? True.

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But in the numerous resolution you will find a main difference, according to that, which is sought in both, noted with a straight line over the head.

$$4OA^2 - OA^4 = \overline{OB}^2 \text{ duplicatio}$$
$$4\overline{OA}^2 - \overline{OA}^4 = OB^2 \text{ subduplicatio}$$

et similiter pro subtriplat. et subquintuplat.

Let this be remembered, that whosoever will rightly study my book, that it may be a Clavis to him, he must be attentus, operans, constanterque per ipsa vestigia insequens. Non enim oscitantibus scripsi, sed vere Matheseos candidatis. Pressa sunt magis quam brevia.

Stokes's letter, with Oughtred's reply, is printed in the Gen. Dict. vol. viii. p. 85. The pages referred to in it are those of the third edition of the Clavis which came out 1652. Stokes's fourth inquiry has not much difficulty. It refers, however, to a curious subject. Oughtred finished his Clavis in 1631 by this problem of finding the multiple or aliquot part of an angle, which he effected by the relative lengths of their chords. This gave him a formula for the sines of multiple arcs, which is different from that which was the subject of Dr. Twysden's letter (see XXV.). OA, OB, OC, being the chords of arcs which are to one another as 1, 2, 3, he shews that $\frac{4R^2.\,OA^2 - OA^4}{R^2} = OB^2$. and $\frac{3R^2.OA - OA^3}{R^2} = OC$. Hence if the arc subtended by OA be 2A and radius be taken as 1, we shall have in the first case $16 \sin.^2 A - 16 \sin.^4 A = 4 \sin.^2 2A$ or $2 \sin. A\,(1 - \sin^2 A)^{\frac{1}{2}} = \sin. 2A$: and in the second $3 \sin. A - 4 \sin.^3 A = \sin 3A$. In the same manner (omitting the radius) he shews that $5OA - 5OA^3 + OA^5 =$ the chord of $10A$; or $5 \sin. A - 20 \sin.^3 A + 16 \sin.^5 A = \sin. 5A$.

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XXXII.

WALLIS TO OUGHTRED.

Sir,

In that of mine to you, sent together with an account of an eclipse observed at Oxford last summer, (which I hope is come to your hands,) I made bold to propose a problem, which then came newly to my mind, (though that, in order to which I proposed it, had been longer thought of,) which, as I remember, was to this purpose.

Expositis, A, B, C, D, E, &c. (intellige, æquabiliter crescentibus, secus enim propositio infinitarum solutionum capax est:) quarum rectangula $A.B = 1$, $B.C = 2$, $C.D = 3$, $D.E = 4$, &c.

Quæritur quantæ sunt ipse A, B, C, D, E, &c.

That you may the better understand what I aim at, I thought it not amiss to send after it this other, which by that former I hoped to get resolved, viz.

Si sit æquabilis curva (non hinc inde subsultans) AC, cujus axis AX, et tangens in vertice AT, unde ductis ad curvam rectis, axi parallelis et æqualibus distantiis remotis, harum secunda, quarta, sexta, octava, decima, &c. (in locis paribus) sint 1, 6, 30, 140, 630, &c. (sive 1, $1 \times 4\frac{2}{1} = 6$, $1 \times 4\frac{2}{1} \times 4\frac{2}{2} = 30$, $1 \times 4\frac{2}{1} \times 4\frac{2}{2} \times 4\frac{2}{3} = 140$, $1 \times 4\frac{2}{1} \times 4\frac{2}{2} \times 4\frac{2}{3} \times 4\frac{2}{4} = 630$, &c. vel etiam 1, $1 \times \frac{6}{1} = 6$, $6 \times \frac{10}{2} = 30$, $30 \times \frac{14}{3} = 140$, $140 \times \frac{18}{4} = 630$, &c.) Quæritur, quantæ sint prima, tertia, quinta, septima, nona, &c. (in locis imparibus). Saltem quanta sit tertia?

The reason of the affinity which I take to be between this and the former, is because these terms in locis paribus (supposing the second to be 1) are made

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up by continued multiplication of these numbers $1 \times \frac{6 \times 10 \times 14 \times 18, \&c.}{1 \times 2 \times 3 \times 4, \&c.}$ or $2 \times \frac{12 \times 20 \times 28 \times 36, \&c.}{2 \times 4 \times 6 \times 8, \&c.}$ And (if I mistake not in my conjecture), supposing the first to be Q, the rest in locis imparibus will be made up by continued multiplication of these numbers $Q\,\frac{8 \times 16 \times 24 \times 32, \&c.}{1 \times 3 \times 5 \times 7, \&c.}$ which I thought it requisite to give you notice of, that you might see how far I had proceeded towards the solution of what I seek. What I aim at is the true nature of the curve line proposed, and how to describe it by points, or how to assign any point thereof required; at least, what is the length of the third of those lines parallel to the axis, wherein if you can do me the favour to help me out, it will be a very great satisfaction to me, and (if I do not delude myself) of more use than at the first view it may seem to be. Sir, I doubt, I presume too much upon your goodness, in offering you this trouble: wherein I must crave your pardon. If your occasions will permit you to bestow some thoughts thereon, you will thereby very much oblige,

Sir,

Your very affectionate friend,

and humble servant,

JOHN WALLIS.

Exeter College in Oxford.
Feb. 28, 1654–5.

The beginning and ending of this letter are printed in the Gen. Dict. vol. viii. p. 86; but the problem is omitted. It is the last of Wallis's Arithmetica Infinitorum, where the diagram illustrating it may be seen. See also Wallisii Opera, vol. i. pp. 359, 362.

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XXXIII.

WALLIS TO OUGHTRED.

This is the Dedication to Oughtred of the Arithmetica Infinitorum, which it did not seem advisable to reprint apart from the work to which it belongs.

XXXIV.

OUGHTRED TO WALLIS.

Most honoured Sir,

I have with unspeakable delight, so far as my necessary businesses, the infirmness of my health, and the greatness of my age (approaching now to an end) would permit, perused your most learned papers, of several choice arguments, which you sent me: wherein I do first with thankfulness acknowledge to God, the Father of lights, the great light he hath given you; and next I gratulate you, even with admiration, the clearness and perspicacity of your understanding and genius, who have not only gone, but also opened a way into these profoundest mysteries of art, unknown and not thought of by the ancients. With which your mysterious inventions I am the more affected, because full twenty years ago, the learned patron of learning, Sir Charles Cavendish, shewed me a paper written, wherein were some few excellent new theorems, wrought by the way, as I suppose, of Cavalieri, which I wrought over again more agreeably to my way. The paper, wherein I wrought it, I shewed to many, whereof some took copies, but my own I cannot find. I mention it for this, because I saw therein a light breaking out for the discovery of won-

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ders to be revealed to mankind, in this last age of the world: which light I did salute as afar off, and now at a nearer distance embrace in your prosperous beginnings. Sir, that you are pleased to mention my name in your never dying papers, that is your noble favour to me, who can add nothing to your glory, but only my applause, and prayer that God by you will perfect these happy beginnings so propitiously advanced to his glory. Which is the hearty desire of

Your [truly loving friend
and honourer,
[Aug. 17, 1655.] WILLIAM OUGHTRED.]

This is the letter of which Wallis has published a Latin translation (Opera, vol. i. p. 363). It is here printed (as it was in the Gen. Dict. vol. viii. p. 86) from the copy which Oughtred had kept by him; but the original is in the Savilian library, from which the signature and date have been added. There is one sentence here, which differs a little from what was actually sent to Wallis, where it stood thus: "the learned patron of sciences, Sir Charles Cavendish, shewed me a written paper sent out of France, in which were some very few excellent new theorems," &c.

XXXV.

R. SHUTTLEWORTH TO OUGHTRED.

Sir,

Pardon me, I pray you, my forgetfulness, that till now I have not returned you my real thanks, not only for your last letter, but for your great pains in answering mine by examples of your own, and the manner of working them. And such your love and pains give me boldness to put you two other questions, the resolution and operation whereof I desire to know by Equation; for by knowing and seeing the work

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(though I confess they are too trivial to trouble you with) of these questions, the manner of answering divers other questions, by the said rules of Equation might (I think) be easily attained. The examples or questions at large are as followeth:

A, *E*, and *I*, reasoned thus:

A said, if I had £480 more than I have, I should have as much as both *E* and *I* have; *E* said £480 added to mine would be twice as much as *A* and *I* have; *I* said £480 added to mine would be thrice as much as both *A* and *E* have.

How much had each of them?

The second question.

What three numbers are those, that the first $+ 73 =$ twice the second and third; and the second $+ 73 =$ thrice the first and third; and the third $+ 73 =$ four times the first and second.

The third question I do presume to trouble you with is the extraction of the cubic root of 0.75.

Sir, I know you can, and so hope you will give satisfaction to these questions, and so shew the operations of them severally, or else no man can; for no man living knows more than you in the mathematics; and though you are famous for skill in this nation, yet you are, to your honour, more famous and more honoured in other foreign parts beyond the sea. And as I make my only address to you herein, so give me leave to say, you must either please to resolve me of these questions, as I doubt not, or else I shall undoubtedly think I can have no other person to resolve them, and I pray you will give me this satisfaction to each one of them as speedily as you can; and if your leisure and conveniency would permit, I could much desire it by the end of the term. Your last by your care came safe to my hand, and I hope your answer

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to these will do the same. Both I and William Sudell, your scholar, think it an unhappiness that we are not near you, or else we should be still learning something of you, though we are able but to glean after your harvest. Well both William and I wish you long life to honour your name more and more; and so in haste I rest,

Your real friend and servant,

Westminster, this 22nd of Jan. 1656.

RIC. SHUTTLEWORTH.

If you indorse your letter thus:

For Col. Richard Shuttleworth, at his lodgings at Mr. Bryan, a sadler, his house in Tothill street, a little beyond the Gatehouse, at Westminster, these.

And below the superscription you may please to write,

Leave this with the gentleman porter of Arundel House in the Strand.

And I doubt not but he will take care to send it to me, as it is so directed, and which he did the last you sent: for it came safe to me without delay.

Oughtred has written the solution of the first question, and the extraction of the cube root, on the back of this letter, and has added, "The first question resolved with whole process, whereby also the second question may be wrought, (both being of the same nature), which therefore I leave to the solerty of W. J. for ἔργα νέων." It is curious to see the importance, which could in early times be attached to such problems, but the details of their solution are no longer of any general interest.

XXXVI.

WILLIAM OUGHTRED TO SIR CHAS. SCARBURGH.

Dignissime juxta ac
Benignissime Mecænas,

Humanitatem, qua me nuper excepisti (ignotum

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licet, et immerentem) quotiescunque recolo, toties grata veneratione procumbit animus. Hæreo sane, an memet audaciæ insimulando magis sim, an potius benignitatem tuam tam libere in me collatam agnoscendo. Te quidem in excelso fastigio licet generis et eruditionis positum, tam comiter tam blande excipientem sensi, quam memet tanti viri familiaritatem (parcas audaciæ) impensius desiderantem. Aureis (quibus me donaras) recens proculdubio tui virescet memoria; nec tam flaccescentis styli indigebit μνημοσύνῳ: patiatur tamen humanitas balbutientis genii (vestram dum pingit beneficentiam) dictamen. Dono ditasti vere regio; in quo dubium, an inscriptam Cæsaris imaginem an Augustam regiæ liberalitatis faciem clarius intueri liceret. Irriguo benevolentiæ tuæ perfundor imbre, ad tui amoris radios pectus incalescit: at una hausi animo novam velut flammam, cujusmodi in Vestæ penetralibus virginum olim religio coluit, hoc est perennaturam. Sunt quidem ex officialibus Telonio Regio servientibus, quos non modo remissius et incuriosius negotium suum gerentes, verum etiam muneribus sordide corruptos, negotiatorii vectigalis Præfecti suo munere abdicarunt: in quorum vices, te mihi aspirante, et suppetias ferente, me substitutum iri spero. Nec enim candor tuus petentem aversabitur, qui sublato olim ad sperandum vexillo, suggessit ut orarem. Obstrictum me fateor; at thure digno tuæ benignitatis aræ gratias libare nescio. Verum et Diis lacte rustici, multæque gentes supplicant; et mola tantum salsa litant, qui non habent thura, nec ulli fuit vitio Deos colere quoquo modo possent. Tibi quando gratiam referre ut meritus es de me nequeo, has litteras (donec occasio detur majore quam verbis officio remunerandi) confessoriæ tabellæ ac velut pignoris loco te habere quæso. Quod tamen etiamsi in se indignum

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tua benevolentia et candore, potest dignitas, non modo non acceptum habere, sed et accipiendo dignum facere. Quod si tuæ beneficentiæ visum fuerit priorum cumulo adjicere, in cœlum abire videor.

Tibi Devotissimus,
GUIL. OUGHTRED.

Ex Ædibus Maternis
xi. Calend. Jan.

This letter is in the firm and rather formal hand of a young man, which is quite different from that of Oughtred himself. It must have been written, after his death in 1660, by his son William, who is mentioned by Huniades. See p. 30. Besides, it is addressed to "Sir Chas. Scarburgh, knt.," who did not attain that rank till after the Restoration.

XXXVII.

HUGENS TO SIR R. MORAY.

Sir, Hague, July 14, 1662.

I hope you have received my last, which I had the honour to write you about two or three weeks ago. This is especially to entreat you to thank Mr. Boyle in my name for his present, which I even now received, of his last book [u], that was delivered [to] me the other day, not by him, whom he had charged with it, but by another from him: his businesses not permitting him to come yet to the Hague. I was at first astonished to see that he has taken the pain to write so big a book against objections so frivolous as those of his two adversaries, but having begun to peruse it, and seeing that among his refutations he has inserted

[u] Defence of the Doctrine touching the Spring and Weight of the Air, against the Objections of Fr. Linus. And an Examen of Mr. T. Hobbes his Dialogus Physicus de Natura Aeris as far as it concerns Mr. R. Boyle's book. 4to London. 1662.

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many new discoveries and observations not yet seen, I wished it had been bigger. I was, above all, very glad to find in it both the experiments concerning the condensation and rarefaction of the air, which prove clearly enough that remarkable property, viz. that the strength of its spring follows the reciprocal proportion of the spaces wherein it is reduced. When I consider this, I find more difficulty than ever to give a reason for what is seen in my experiment of the water freed from air, which descends not in the reversed phial, though the receiver wherein it is be voided of air. I have writ to you thereof heretofore, but without many circumstances, because I thought that you would have learned them enough in the making of it; but being I understand not yet that you have taken the pains about it, I send you here the whole history of it, where you shall see something that will surprise you, and which deserves to be considered.

Having yet but perused Mr. Boyle's book, I could not take notice of all the fine things which it contains; but where I have read, I see appear much spirit and modesty, with that ordinary retinue, which keeps him from speaking definitively, as most of your present philosophers do. He has judged very well that Linus his first argument was the chiefest thing in his book, and solidly resolves what the other opposes of the attraction of the finger in the siphon opened at both ends; but there remains one difficulty with [me[v]] by this experiment, in which I do not see that either Linus's hypothesis or Mr. Boyle's is satisfactory, that is, why the siphon sticks to the finger, so that one must use some little force to draw it off. For I conceive well that the finger being pressed from above with the weight of the atmosphere and the mercury

[v] Mais il me reste une difficulté dans cette experience. *Original.*

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from below, they ought for that reason [to] remain joined together, but not what hinders the siphon itself to descend, if it be true that that happens, though it be of glass very thin and a little immerged in the mercury: for it is not pressed stronger from above than from below, but on all sides equally: whence comes it then that it must be drawn to make it leave the finger? for one cannot say that this keeps it, nor also the quicksilver. The hypothesis of the springs of the air is very ingenious, and satisfieth unto most part of the phenomena; there is only this, which I cannot tell how one cause join it, viz. that the air though compressed in a vessel keeps its fluidity; for when one imagines this vessel full of such springs touching each other and a great weight above, that presses them, it seems that they could not yield easily to a body that would pass through it. If it be the inward circular motion, which Mr. Boyle supposes is given them, which preserveth their agitation, it must almost be such that it cannot be stopped by any means soever, which is a little hard to digest. But I know that he gives this hypothesis only as [a] project, and specially to bring a possible means of the expansion of the air. I am sorry that in the experiment he has made on the top of Westminster, there is that inconvenience of heat and cold, which hinders the exactness thereof; one should try to prevent it by encompassing the phial AB[v] with water where it should be immersed to the neck; for in that little time that must be to descend it, this water cannot change its degree of heat; and if one could thus come to some evenness in that experiment, it would be of importance. You will tell me perhaps why I make it not myself, with many other of the same nature. I have given you the

[v] Defence, &c. p. 51.

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reason why in my precedent, and notwithstanding for my own curiosity I can possibly make some speedily, for I have some occasions to be in one of our cities where there is a very fine glasshouse, where I can be provided with vessels of all sorts. You have doubtless seen Mr. Hevelius's book of Mercurius in sole with the treatise of Horroxius [w], therefore I say nothing of it, and end, &c.

Experiment made in Feb. 1662.

For this experiment Hugens took a glass tube open at one end and blown out at the other into a sphere. This having been filled and set with its mouth downwards immersed in a vessel of water, was placed under the receiver of the air pump. When the exhaustion was produced, Hugens found, that, if bubbles of air were thrown out from the fluid, they expanded and the surface sunk, but when the water was well freed from air, he was unable to make the water descend from the top of his tube. Not being aware that this was the consequence of the imperfection of his instrument, he imagined that it might indicate some peculiar property, which, under these circumstances, belonged to the water.

At the meeting of the Royal Society on the 23rd of July, 1662, "The amanuensis was ordered to translate from the French Mons. Huygens's letter to Sir Rob. Moray," of the 14th of July, 1662. An extract from the original was entered in their Letter Book, and this is probably the translation which was made from it.

XXXVIII.

HUGENS TO SIR ROB. MORAY.

Sir, Hague, 18 August, 1662.

I have received, by Mr. Kilpatric, yours dated of

[w] Venus in sole visa, the manuscript of which was lent for this publication by Hugens to Hevelius.

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the 7th of July, with two new books, for which I thank you most humbly; and I had done it sooner if I had not been absent two ordinaries. That[x] of Mr. Evelyn is learned, and of very great search, and I have had much satisfaction in the reading of it, without that of finding me so honourably inserted in it. I had scarce believed [one] who had told me that I should find my ring of Saturn mentioned in a book of so different a subject. I remember very well to have had the honour to see the author in your chamber more than once, but I am not very certain if it was he, who promised an ample treatise of Gardening. If it be so, I doubt not but it will be a work most accomplished and curious. It seems to me that I comprehend sufficiently Prince Rupert's new method[y] when I consider that head of his making, and the little overture Mr. Evelyn has given us of it. It is a marvellous abridgment in things where there is much shadows, and makes a most fair effect.

What shall I say to you now of Mr. Hobbes's book? By his abundance of absurdities he becomes pleasant, and I know not if I do well to contribute to bring him to silence hereafter, in condemning his paralogisms. Yet because you will have it, I send to his bookseller my judgment of his duplication of the cube and the quadrature [of the circle], where I shew clearly enough wherein he is mistaken, as you shall see by this copy.

I see that you speak but of one letter you have received, which causes me to doubt if that, which I have written to you since, has been delivered [to] you; but

[x] Sculptura, or the Hist. and Art of Chalcography. 8vo. London. 1662.

[y] Mezzotinto, of which the first specimen is a head at p. 145 of Evelyn's book: Prince Rupert's initial R, with a coronet over it, is in the upper corner of the plate.

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perhaps it has been longer on the way than it should. It was chiefly to entreat you to thank Mr. Boyle from me for his last book, which he presented me with, and I had added to it a pertinent description of my experiment of the water purged from air, which descends not in the vacuum, wherein there are remarkable circumstances.

After I had sent that last letter I have meditated on the cause of the extension of air, and I have found, first that the height of the atmosphere is infinite, it being supposed true what Mr. Boyle's experiment seems to prove. And, moreover, I have framed a rule most easy to know, in a height given, how much of the weight of the air is yet from thence above. As, for example, being on a hill high of 22873 feet of London, one shall have yet a half of the said weight above one's head; and if one attained to the height of 380010 feet, that one shall have but $\frac{1}{100000}$ of air upwards.

I suppose, concerning the gravity of air here below, that it is to that of water as 1 to 970, as thereabout I have found it by my experiments, and that a cylinder of water of 34 feet counterbalances a cylinder of air to the end of the atmosphere, as the experiment teaches it. The rule is such as this is.

Let the height given be of 22873 feet;

$$\text{add}\begin{cases}4.35933 & \text{logar. } 22873\\ \underline{0.11927} & \text{numerus semper addendus}\end{cases}$$
$$4.47860 \quad \text{huic logarithmo convenit}$$

$$\begin{array}{rll} \text{numerus} & 0.30103 & \text{quem subtrahe ex} \\ & \underline{5.00000} & \text{logar. } 100000 \\ \text{reliq.} & 4.69897 & \text{logar. } 50000 \end{array}$$

As then 100000 to 50000, or else as 2 to 1, so shall

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be the weight of the cylinder of the height of the whole atmosphere to its part upwards from the height given.

Again, let the height given be of 100000 feet:

add { 5.00000 logar. 100000
0.11927 numerus semper add.
5.11927 huic logar. convenit

numerus 1.31600 quam subtrahe
ex 5.00000 logar. 100000
3.68400 logar. 4841.

The weight of the whole cylinder to the part upwards will be then as 100000 to 4841, or almost 21 to 1.

As to the contrary, when the weight of the air upward is given, and that one must know the height of the place, you must use the following rule, of which the foregoing is drawn. For example. I would know, to what height one must attain, to have $\frac{1}{10}$ of the weight of air above your head:

subtr. { 1.00000 log. 10
0.00000 log. 1
1.00000, diff. logarithmorum.

5.00000 log. ejus differentiæ
0.11927 numerus semper addendus
4.88073 logar. 75986 qui est numerus pedum altitudinis quæsitæ.

Though I believe not that the extension of the atmosphere be infinite, yet I am of opinion that one mistakes very little in using these rules, which I entreat you to let my Lord Brounker and Mr. Boyle see.

I have some fear that this arrives but after your departure from London: but I can be easily com-

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forted, because I shall have the sooner the contentment to see you here, which will be one of my greatest that I can wish for.

I am, &c.
CHR. HUGENS,
of Zulichem.

At a meeting of the Royal Society on 20th of August, 1662, the amanuensis was ordered to translate into English, from the French, M. Hugens's letter to Sir Rob. Moray, dated from the Hague the 18th of August. The extract in the original language is entered in their Letter Book, and this (like XXXVII.) is probably the translation to which reference is made.

XXXIX.

DARY TO COLLINS.

Sir,

Having the opportunity of so convenient a bearer as Mr. Mortimer, your loving friend and associate, I took the boldness upon me to present you with a line or two: first by way of intimation, and then by way of request.

First, I having (by your good-will and good word) come down to the city [of] Bristol as a guager, I have considered the form of those tuns or vessels which the brewers do use, and I cannot tell by what appellation rightly to call them, for most of them are neither frustums of cones nor cylinders. I thought to call them tri-surface or tessera-hedrum, but neither of these names pleased me, so 'till I have better advice from you I call them cylindroids (by which I mean) a solid contained under three surfaces, viz. two planes parallel called the bases, and these bases are either both circles

or both ellipses, or else one a circle and the other an ellipsis, and the third superficies is convex begirting the whole body, which I call the zone, in which zone a right line may be everywhere applied directly from base to base.

Now to find the content of a solid according to this definition, (which doth take in the frustums of cones and cylinders, for they will fall under this definition,) I have attained to an excellent general theorem which is very demonstrable, by which I have had abundance of contemplative solace and satisfaction, and not only so, but an incredible facility and celerity in calculating the contents of those vessels from inch to inch. For I do it all by addition of the primes (or heads) of the first, second, and third differences. And it is so much the more worth, because it is of frequent use.

Secondly; as for the second sections of a spheroid, (I could wish with you that it were both true and communicable, but I am possessed with a timerity that it will be both together; for as yet I cannot make the unknown symbol stand in any equation but it destroys itself. Howbeit in the method of indivisibles I have two ways for it, but they are so intolerably tedious that they are not here to be mentioned, therefore) my request to you is that you would let me know how that Dutch author doth it by his tables, and that at your conveniency you would let me know your geometrical way of finding the foci in the conical sections: my request is also that you would present my respectful duty to grandsire Reynolds and my father Bond, and my love to Mr. Leak, Mr. Harvey, Mr. Sutton, Mr. Ruley, and all the rest of our brethren as if I had named them in particular. Not to trouble you further, but my respects to yourself and Mrs. Collins, your second self, I shall only tell you, that if there

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be any thing wherein I may serve you at Bristol, it shall be endeavoured by

Your engaged friend,
MICH. DARY.

Bristol, the 23rd
April, 1663.

These lines I did intend to send by Mr. Mortimer, but he being gone I was fain to make use of the post.

XL.

SIR WILLIAM PETTY TO ——

Sir,

I have been acquainted with the opportunity you took to restore me to his Majesty's good opinion, and do heartily thank you for that endeavour, being resolved to be patient until it succeed, which I can very well do, because no sense of guilt troubles me, and withal can (as I every day do) labour and spend myself for his Majesty, as for my friend as well as my sovereign. When I have lost the rest of my estate here, which will quickly be, I shall be forced to come over, and then I promise myself one occasion or other to let his Majesty know how often and maliciously, and upon what design, I have been wronged and traduced, and should be glad to have my accusers present. Till then I can be patient; only I assure you in the mean time, and if you dare venture to assure his Majesty (you shall not miscarry in it) that I never, in my whole life, did or said any thing to his prejudice, but much to the contrary in the worst times, and as for the particular at which his Majesty hath taken offence, I shall demonstrate a vindication;

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and perhaps before I come, time and the issue of his affairs in Ireland will justify the most offensive of my actings, and shew that my accusers could not, by the sudden inspiration of their cups, know what many years meditation hath taught me as to the interest of Ireland.

As for the ship design, I have been now eighteen months continually upon it, I can say for no other end than to do a thing grateful to the King. What discouragements or non-encouragements I have met with you know well, but not the tenth part, yet I am as cheerful as ever. The vessel of ten guns I mentioned in my second letter to you is near finished, to shew you that I make hard shift to perform whatever I but talk of. I had not my full liberty in this work, and therefore I do not fully approve her, yet I have hazarded fifty pounds against all the vessels in the world, to run against her from hence to and back from Holyhead in Wales, upon the 20th of July, be the wind or weather as it will, which I think is a searching and sound way of proof. When we are safe in the mechanical and natural part, I shall in a few minutes satisfy his Majesty in the political, and if he please but to venture in that way proportionally to what I have done in t'other, may quickly reap the fruit. The consideration of double bodies hath (unless I am become drunk with too much thinking) brought me to the perfection of the shape, that the single body of any assigned dimensions must be put into for its most easy passage thorough the water, to the very utmost of what in nature is possible. But why do I begin to talk of these things? for before I can satisfy your senses, to do so is but mountebankery; and then we shall not be free from heterogeneous objections to

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counterpoise the merit of these labours. I have of late examined the common way of building, viz. the way of draughts and models, and I desire you to ask Lord Brounker (to whom I pray present my most humble service) whether there be any thing in it, but conjectures and concinnities, &c. viz. whether a small vessel (I do not say a great one which the eye cannot comprehend all at one view) may not be built as well by the eye as by a draught or model, or rather, whether a log of wood might not by the eye be carved into that shape which a shipwright would think best. I hope I have abolished all these guesses and uncertainties, and if I have, quorsum hæc?

Since I wrote this our vessel is finished in all the necessary parts, and hath sailed by and large, to the admiration of some hundred seamen and others, for swiftness, keeping a wind, small draught, not stooping, staying and steering; yet I must tell you, most of these admirers put in their diminutions; some, that 'twould quick part asunder; others, that it could not work in a grown sea, &c. This night she put to sea in earnest with intention [to] try all sorts of experiments in the main sea. I hope to have a good account of her, though doubting and diffidence be very natural to,

Most honoured sir,
Your most affectionate
faithful and humble
servant,
Wm. Petty.

Dublin,
8 July, 1663.

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XLI.

HUGENS TO SIR ROBERT MORAY.

Paris le 11 Novembre 1663.

Je vous suis fort obligé des bonnes nouvelles, qu'il vous a plu me communiquer touchant le comportement des pendules par mer, et j'en ai beaucoup de joie, ne m'ayant pas osé promettre qu'elles auroient un si bon succès. Ne manquez pas, je vous prie, à m'envoyer au plutot la relation du capitaine, tant pour m'éclaircir entierement en ce qui regarde cette importante expérience, qu'à fin qu'il m'en puisse servir où il sera besoin; car je suis d'avis ainsi que vous, et ceux de la Société Royale, qu'il faut commencer à agir tout de bon dans cette affaire, et qu'il y a assez de fondement pour demander sans hésiter les privileges. M. l'Abbé de Beaufort, avec quelques autres de mes amis, me conseillent tous de demander plutot une récompense ici au Roi qu'un privilege, et proposent meme des moyens, dont il faudroit se servir pour l'obtenir. Pour moi, je crois que ce ne seroit pas mal, mais je désire d'en savoir votre avis, et j'en consulterai cependant avec d'autres personnes encore, que je sais m'y pouvoir servir. En Hollande le meilleur sera de demander le privilege, et l'affaire réussissant bien, le prix, qu'on y a destiné, ne pourra pas nous manquer. Pour l'Espagne, le Danemarc et la Suede, je sais des gens, que j'y pourrai employer. Ayons donc votre journal, parceque par tout il faudra l'alléguer. M. le C. de Kincardine en sera aussi bien réjoui, et il me tarde de savoir sa réponse, et d'entendre son avis sur le tout. Je lui suis bien obligé de ce qu'il a toujours eu meilleure espérance touchante cette invention que moi. Toutefois, si mon voyage ne fut pas survenu, je l'avois déja si bonne, que je me serois

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embarqué avec deux horloges, que j'ai fait faire pour cet effet, et qui m'attendent maintenant à la Haye. Je n'entends pas bien ce que vous dites de la différence de 15 degrès, qu'on a trouvée; est-ce entre la longitude trouvée par la voie ordinaire et par celle des pendules? Ce seroit un peu beaucoup, et j'aurois peur que les pendules n'auroient pas été mises à la juste longueur, à quoi il faut prendre garde surtout, et je vous prie de la recommander à celui des votres, qui aura soin d'accommoder les horloges, quand ils s'en vient aux Indes. Il faudra bien aussi donner instruction au capitaine, pour ce qui est de la méthode de trouver l'heure au lieu, où il est, par le moyen de deux égales hauteurs du soleil devant et apres diner, car autrement je sais qu'ils ont des manieres d'observer le midi fort peu exactes. Pour l'équation du tems je veux croire qu'il y aura pris garde en se servant de ma table.

L'expérience[y] du mercure, qui demeure à 55 pouces sans descendre, est tres belle, et établit sans contredit le paradoxe qui paroit étrange à ceux, que j'en ai parlé ici, et qui sont des plus subtils. Je vous prie que je sache, comment ils ont pû si bien purger le mercure de tout air, s'il n'a fallu que le laisser comme on fait à l'eau, ou s'il y faut autre chose. Que je sache aussi, si les 55 pouces sont demeurés, quoique le récipient fut vuidé de l'air, ou seulement, auparavant, car c'est déja un miracle, puisqu'il ne demeuroit ordinairement qu'à 30 pouces au plus. Les observations des changements du tems, que vous avez ordonnées, seront assurément belles, et de plus d'utilité que l'on ne croit. Quand je serai en lieu de repos, je vous prierai de me procurer un thermometre comme ceux que vous avez, ou, du moins, que vous m'en communiquiez la fabrique, s'il y

[y] By Lord Brounker. See Birch. Hist. R. S. vol. i. p. 320.

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a quelque chose d'extraordinaire à ceux, que fait votre opérateur. Je vous assure que toutes ces belles expériences et occupations de votre illustre société à tous ceux que j'en parle, font avoir grande opinion et respect pour elle.

This extract is inserted in the Letter Book of the Royal Society.

XLII.

HUGENS TO SIR ROB. MORAY.

Monsieur, Paris le 18 Nov. 1663.

J'ai considéré l'extrait du journal, que vous m'avez fait la faveur de m'envoyer, lequel me confirme beaucoup dans l'expérience [espérance?] que votre précédente lettre me donna du bon succès de nos pendules sur mer. C'est déja un grand point de savoir qu'une horloge bien faite, comme est celle de la Haye, peut supporter les plus grandes tempetes sans s'arrêter: et quant à la justesse, il n'y a rien dans ces observations faites au retour de Lisbonne, qui empêche de croire qu'elle n'est été exacte, puisque les longitudes prises par la voie ordinaire se rencontrent assez bien avec celles qu'a montré l'horloge; si non en l'observation du 26 Aout, où l'une donne 9° 22′ de longitude, et l'autre 11° 15′, dont la cause peut être attribuée au peu de certitude qu'il y a dans la maniere ordinaire, ou bien plutot ici à quelque erreur, qu'on aura commise à prendre l'heure, de sorte que l'horloge n'aura dévancé le soleil que de 37′ ou 38′ min. au lieu que par leur observation il y en eut 45′. Car ce qui me fait juger ainsi, c'est qu'aux trois observations suivantes et dernieres les longitudes de l'une et l'autre maniere s'accordent derechef tres bien. Pour avoir une preuve certaine de la justesse des horloges, il faudroit mesurer la différence des lon-

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gitudes de deux lieux, en allant et en venant, pour voir si l'on trouveroit la meme. Il semble que votre capitaine n'y a pris garde qu'au retour, dont peut être j'aurois appris la raison, si vous m'aviez envoyé le journal entier, en tant qu'il concerne les horloges. Il y a encore d'autres particularités, pour les quelles je souhaiterois le voir, comme de ce qu'il parle, aux observations des longitudes depuis le 19 Aout jusqu'au 5 Sept. des deux horloges conjointement, quoiqu'auparavant il ait remarqué que depuis le 25 Aout jusqu'au 4 Sept. l'horloge B passa devant A par trois divisions par jour. Je voudrois aussi savoir pourquoi l'on n'a pas continué les observations jusques en Angleterre. Que j'aye donc le journal entier, pour m'en mieux éclaircir.

Vous voyez par les premieres observations, qu'il y manque encore quelque chose en l'exactitude des pendules, puisque les excès n'ont pas été tous les jours de meme, ce que j'ai observé aussi bien sur terre avec ces memes horloges, lorsque M. le C. de Kincardine et moi étions apres à les ajuster, et je suis bien aise de voir que sur mer ils ont allé tout aussi bien que dans ma chambre. Toutefois ces inégalités m'ont toujours fait juger que les horloges ne nous donneroient jamais les longitudes dans la derniere perfection, mais que néanmoins elles ne laisseroient pas d'y servir beaucoup, et que peu à peu l'on en perfectionneroit l'usage. Il vaut bien la peine cependant de demander les privileges, et qu'on travaille au plutot. Votre pays et le notre sont ceux, où il y aura le plus de profit à faire. Pour celui-ci l'on me soutient que le privilege doit être mon pis aller, mais qu'en tout cas je pourrai facilement l'obtenir. J'attens ce que le C. de Kincardine sera d'avis que l'on fasse. Il sait de quelle façon nous sommes convenus entre nous touchant le partage des émolumens, et il

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peut s'assurer, que je le garderai religieusement de quelque maniere ou de quelque coté qu'il m'en viendra.

L'expérience derniere de M. Boyle me plait fort, parcequ'elle se fait si aisément. Je voudrois savoir s'il ne laisse pas ce peu d'eau par dessus le mercure se purger d'air pendant une nuit, car autrement je ne conçois pas, que par la seule répétition de l'expérience de Torricelli le mercure demeurera suspendu. Vous m'en informerez, s'il vous plait, et de plus, si apres que le mercure est descendu dans le tuyau, où il a demeuré douze heures, si dis-je il ne s'arrête à la hauteur ordinaire de 29 ou 30 pouces. Quand je saurai cela, je me mettrai tout de bon à méditer sur la cause de cet étrange phænomene.

Ce que j'ai à vous dire touchant les lunettes d'approche, que les curieux d'ici fabriquent, c'est que dernierement nous fimes l'essai d'une de 35 pieds sans aucun tuyau, qui réussit admirablement bien. La façon de dresser le verre objectif est de M. Auzout, et consiste en ce que dans un petit ais de deux pieds environ, où ce verre est enchassé, il ajuste un petit tuyau étroit, justement à angles droits, à travers lequel lorsque celui, qui est aupres, voit l'étoile qu'on veut regarder, l'on est assuré, que le verre objectif est situé comme il faut, et l'on trouve aisément apres cela le lieu pour mettre l'oculaire qui est soutenu par un pied. Vous entendrez ceci assez, autrement je vous l'expliquerai plus au large, comme aussi la maniere, qu'on a proposé pour élever le verre objectif à la hauteur qu'on veut.

M. Auzout a un verre de 45 pieds, qui a $6\frac{1}{2}$ pouces de diametre, et est tres beau et bon.

L'on me mande de la Haye, que l'horloge pour My-Lord Brounker est achevée, et qu'on désire de savoir quelle addresse il y faudra mettre pour le lui faire venir.

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Nous avons commencé de faire des expériences du vuide chez M. de Montmort avec la machine, qu'il a fait faire suivant mon ordinaire ; mais tout cela ne va pas de l'air, comme chez vous.

The sum of the account of Major Holmes concerning the pendulums at sea.

1663	B gained of A daily. Min.	Divis.	B stood.
From Ap. 28 till May 3.	0	4	Stood.
May 3 —— 12.	0	5	Lead fell off and six days stop.
18 —— 24.	0	5	Stood.
24 —— 30.	0	5	Stood and was oiled.
Jun. 3 — Jun. 16.	0	4	Stood.
16 — July 1.	0	4	Stood.
July 1 — Aug. 13.	0	3	Both adjusted at Lisbon.
Aug. 13 —— 25.	0	$3\frac{1}{4}$	Stood.
25 — Sep. 4.	0	3	

1663	lat.	long. from Lisbon westward.	miles.	miles in a day.	minutes before the sun.
Aug. 19	39°10′	4°45′	181	—	15
22	41 7	5 2	234	45	20
24	43 12	7 37	336	44	30
26	46 0	9 22	408	44	45
29	47 0	7 41	315	41	30
Sept. 1	49 0	1 $41\frac{1}{2}$	66	39	little diff. from ☉.

5. After five days' hazy weather the watches were 30′ behind the sun.

The watch called A never stopped all the voyage, though he [major Holmes] was in as great [a] storm as ever he saw.

The two preceding letters are copies, which have probably been made in England, "Aug." having in one instance been written by mistake for "Aout." They are in the collections of the Royal Society, where there is also a subsequent letter of Hugens, in which he accuses the E. of Kincardine of endeavouring to deprive him of his just right to the discovery,

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XLIII.

HUGENS TO SIR R. MORAY.

Sir, Hague, Oct. 10, 1664.

At the end of your last of Sept. 9, you promised me the other half of your answer by the next post, which is not yet come. In the meantime I much desire to see your instrument for the measuring of descending bodies, and wherein it differs from that whereof I have sent you the figure.

I think I have written to you that I had received the little book of Campani[z], where he relateth the new observation of Saturn, and the wonders of his turn-tool for the making of optic glasses without using any mould. They have lately also sent me a painted figure, which besides the said observation of Saturn, representeth a very pretty one of Jupiter, in the disc whereof they have seen pass the shadows of two of his satellites, which did pass betwixt him and our eyes, and a little after disengaged themselves from the said disc. I had never thought that this observation could be made, considering the smallness of these companions, and certainly their glasses must be of an extraordinary perfection. If that of Reeves of 60 feet is any thing good, it cannot fail to discover the said shadows, when those eclipses happen. I do still expect the diameter of that glass, and that of its aperture.

'Tis very true that 'tis a good while ago that there have been watches of two springs, whereof the great one from time to time winds up the small. But it is quite another thing to make them (à contrepoids) as a

[z] Ragguaglio di due nuove osservazioni, da Gius. Campani. Rom. 8vo. 1664.

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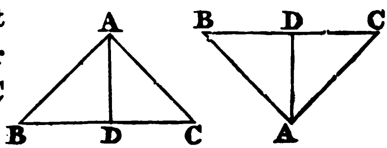
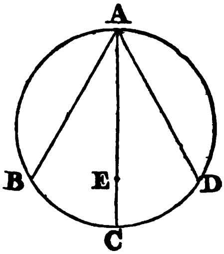
counterpoise, and in such a manner, that whilst the small weight is wound up, it ceaseth not to have just the same force to make the (roue de rencontre) balance wheel turn, upon which it hangs immediately. When you see the invention, you will esteem it more than you do now. The watchmaker hath two of these watches in hand, which are half finished, and whereof one is for you. In the meanwhile give me the pleasure of explicating to me the idea you had conceived for a like engine. Since my last some misfortune was happened to my new watch, which hath hindered me from making observations of its exactness during the time I was in the country, but I have caused it to be rectified since, and they have just now brought it me again. Shall we never hear of those that have been in Guinea?

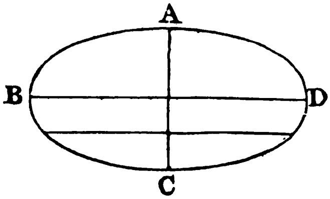
I know not whether my lord Brounker hath since thought on the demonstration of the vibrations of the chord, that are equal to these of a pendulum, which you know: but I found his promise pretty bold. I am lately fallen into a speculation not much remote from that. I have searched for single pendulums, isochronal to triangles and other figures and bodies diversely suspended: where I have lighted upon propositions pleasant enough, and which may even serve handsomely to establish the universal measure, upon which the said lord hath spent some study.



For example, I find that a triangle rectangular and isosceles as BAC being suspended by th top A or by the middle of its base D, and agitated on the side, is isochronal with the simple pendulum of its altitude AD.

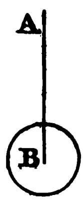


That a circle suspended by a point in its circumference as A, and agitated on the side, is isochronal to the pendulum of $\frac{3}{4}$ of its diameter; and so likewise every portion as ABCD having the sides AB, AD equal.



That an ellipse ABCD, whereof the greater axis hath its square triple to that of the lesser axis, suspended by the extremity of the little axis A, and agitated on the side, is isochronal to the pendulum AC, and so likewise all portions cut by one or two parallels to the axis BD.

If my lord Brounker relisheth these speculations, I will send you more of them, for I have the general determination for all triangles and rectangles, suspended by one of the angles, or by the middle of the sides; item of circles suspended by strings, as is the circle B in A, and (which hath been the most difficult to find) the length of the pendulums isochronal to a sphere suspended likewise by a string, which serveth principally for the universal measure. For you must note, that a great sphere is not isochronal to a little one, which (auroit) shall have the centre equally distant from the point of suspension. The mathematicians in France have heretofore searched after these things, without obtaining what they looked for, as far as I see by the letters, which I have of Pere Mersenne.



I am
Your most humble servant,
CHR. HUGENS DE ZULICHEM.

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N. B. Cane with salt water with a wax ball swimming in it.

This letter was read to the Royal Society on the 12th of October 1664. The present translation is in the handwriting of Oldenburg, who sent a copy of the original to Boyle, as may be seen in the collection of his works, vol. v. p. 312.

Major Holmes's account of his last voyage to Guinea, in which he tried the efficacy of the pendulum watches for finding the longitude at sea, may be seen in the Phil. Trans. vol. i. p. 13.

XLIV.

HOOKE TO BOYLE.

Honoured Sir,

This [i] account having had the honour to be very well approved in the Royal Society, though the experiments contained in it are no other than what I have formerly acquainted you with, yet there being somewhat of new hypothesis, and giving some accounts of the apparatus which is now preparing for the trial of those experiments, and somewhat likewise of the main drift of them, I have added them to th[is] scribble. Mr. Tillotson returns his humble service for the ens veneris you were ple[ased] to promise him, but I have since procured him some very good here in town, which has served the turn, so that I shall give you no further trouble concerning it. I have given Mr. Shortgrave directions for making of a wheel baroscope for you by a new way, which is much more facile than

[i] This letter was written to accompany a paper of Hooke on Gravity, which was read to the Royal Society on the 21st March 1666, and the whole is printed in Boyle's Works, vol. v. p. 546; but the editor has inserted the Dissertation, as if it made a part of the postscript.

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the former, both in making, filling, and rectifying: he shews me likewise some brass pipes, which if they be for injection or transfusion of blood, they would be somewhat better to have small protuberances left at the end, that they may not slip out of the vessel when they are tied on to it; but knowing not the designs of them I could not direct him. I very much rejoice to hear of your return to these parts, and am glad you have made choice of this end of the town, the place I was lately to see, and believe it to be a very good air: 'tis pleasant, private, and there is a very good neighbourhood; and 'tis not full three miles from hence, all over pleasant fields. I do not hear of the death of any of your workmen, save Mr. Thomson and Mr. Shaw the founder; and here are others of the same trade good workmen. I thought to have conveyed this by Dr. Wren, who is this day gone for Oxford, but I was hindered by company this morning. He has something worth your perusal: amongst the rest, a relation of China, new and very good of its kind, though it contain not much of philosophical information till towards the latter end, much of which seems to be transcribed from other[s]. Two or three leaves I have turned down in it on such things as I met with remarkable. But I have already given you too much trouble, and therefore beg your pardon. I am,

Honoured Sir,

Your most humble and most faithful servant,

March the 21, 1665–6. R. HOOKE.

Our collections of rarities at Gresham College is now very well worth your perusal, and I hope to increase it every day. We had yesterday a very full meeting here of the Society, and I hope a greater the next week. I am very glad to hear that you have a sixty

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feet telescope: certainly it may help us to many good discoveries, if it be well made use of. I did, the last week, see an elliptical glass, which in truth did something extraordinary, and more than I had seen before; and I expect shortly to see much better.

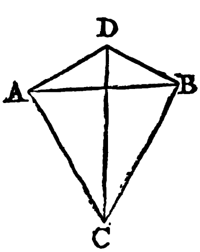
XLV.

COLLINS TO DR. PELL.

Reverend Sir, London, August 28, 1666.

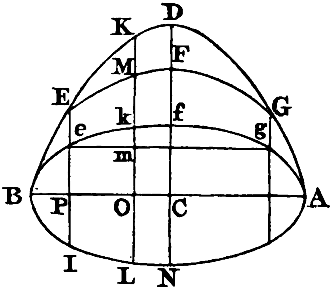
This serves as cover to the inclosed. My lord Brereton says I may presume further upon your patience and goodness than my own inclination leads me: let me, therefore, humbly request your consideration of a few problems that seem easy trigonometrically, but not so analytically.

The four sides and one diagonal of a trapezium given, to find the lengths of the productions of any two of those sides till they concur. This seems to be of excellent use in surveying to take the distance of an object inaccessible without instruments, as of a tree on the other side of the water. This Van Schooten hath not.



Secondly, in the triangle ABC let the sides be given, and let AD be erect or perpendicular to AC, and DB perpendicular to CB; let them be produced to concur, as at D; to find their lengths analytically—may be one question—and supposing either AC or CB a radius, and CD a tangent of some unknown arc to either of those radii, to obtain the tangent or sine of half that arc, and if it might be done without

finding the whole which in projections may excur, it would be of much use.



Thirdly, in the section of a sphere or spheroid AfBNA through its axis parallel to [the] horizon, if to every circle cut erect to axis as that about fCN, kOL you conceive a rectangle equal, whose height shall be an unit, and these rectangles placed on the same planes as those of the circles, one side horizontal, the other perpendicular; if one angular point of those rectangles be in the axis AB, the other shall describe the parabola BDA.

Let now a segment of a sphere as efg be taken off parallel to the axis, and let rectangles whose heights are an unit be conceived to be found equal to the respective segments, for instance a rectangle, whose side is an unit, into MO, is supposed to be equal to the area of the segment, about the segment of the diameter, MOL, and to be placed as the former rectangles were. Then do the angular points of these rectangles describe the curve EMFG. The question is of what nature the curve shall be, aut quæritur locus planus sic describendus.

The lunula EKDGFME × 1 is equal to the segment of the sphere, ekfgme.

And the following compendium I derive from Guldinus for the computation of a segment of a sphere, or supposing the quadrature of a parabola, I can thence derive the same; for the area of the parabolical segment EBPE × by an unit, is equal to the segment of the sphere eBIe.

The proportion is. As $\frac{3}{3.14159}$ or as 0.95493 to the square of the lesser segment of the axis, so the re-

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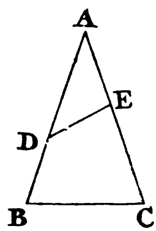
mainder of the axis + radius to the lesser segment of the sphere. And the first number only varies for the segment of a spheroid either erect or parallel to the longer axis in the proportion of one axis to the other.

To fit it for a spheroid cut parallel to the longer axis. As the longer axis to the shorter, so the first number to another that serves for the spheroid, for instrumental use, with a double and single line of numbers, (to operate parabolical proportions instead of 0.95493, I use its root, 0.977, saying as the square thereof is as before.)

The end of this inquiry is to measure the area EKM, and by consequence the solidity of the two segments of the sphere ekme.

A compendium for an upright cone cut elliptically, the plane erect to the triangle through the axis.



As the cube of the side line AB is to the whole cone, so the cube of the geometric mean between AD and AE, to the content of the scalene cone ADE.

The manner how I derive these propositions ex jam demonstratis I am ready to send if desired.

Seeing you are pleased to use > for major, < for minor, what is a good character for an angle?

Having perused some catalogues, I have attained the names of some authors I have not formerly heard of.

Uffenbachius de quadratura circuli 4to.

Ægidii Strauchii tractatus de tabulis et canonibus per mathesim ad facilitandum calculum dispersis[a]. 12mo. The catalogue, I had it from, expresses not the time or place; but I have his treatise de numerornm doctrina. Wittebergæ. 8vo. 1662.

Sanclari apologia pro Archimede et Euclide. fol.

[a] Wittenb. 1662.

Andreæ Alexandri mathemologium. fol.

These the marquis of Dorchester hath, but I would willingly know their character[b].

Mesolabum sive duæ mediæ inter extremas datas infinitis modis exhibitæ per conicas sectiones. Leodii Eburonum 1659. 4to. Authoris nomen subtacetur; perhibetur tamen esse Joannem Slusium canonicum Leodiensem.

Gerard Kinckhuysen wrote an Algebra in low Dutch in 4to, printed at Haerlem 1661, which he intends but as introductory for understanding a treatise of conics he published there the year before, algebraically performed. The author seems very learned, having formerly published a treatise of a Maan Wyser, or an instrument for the moon's theory, and was the first that put that tedious question in Des Cartes of three sticks, and in his Conics hath now published his own geometric and analytic solution thereof.

Bartholinus hath published a Latin 4to treatise of Algebra entitled Dioristice, printed at Copenhagen lately (or divers years since Van Schooten's death). I endeavour after a more exact character of it.

Vouchsafe to give directions about an instrument for parabolas, and Mr. Marke, that succeeds Dutton, will go in hand therewith.

Mr. Pitt sends another sheet on Monday next; and I hope, in my next, to give you a further account of authors. Vouchsafe to account me

Your obsequious servitor,

J. C.

Of this and the other letters of Collins we have only rough copies which he kept by him, and in some cases the persons to whom they are addressed, and the time when they were writ-

[b] The M. of Dorchester is said by Ward, in his lives of the Gresham Professors, p. 91. to have been "not only a patron of learning, but learned himself."

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ten, can only be collected from internal evidence. Spaces sometimes are left in them, for what was intended subsequently to be introduced, which will account for occasional intervals in such parts of the printed text.

XLVI.

COLLINS TO DR. PELL.

Dec. 4.

Branker's gone books [are] put up in a box and left at [the] carrier's; but the carrier cannot bring them this week. Mr. Hake left a draught of the city, but I have seen a much better sent out of Holland. Mr. Pitt hath sent sheet T, and I have already put into the box as much of the second book of Galilæus as I can procure, which I had of Leybourn for a small desperate debt of about 14*s*., and hope to put in three Al[manac]s, one for his lordship, the other for yourself, the third for Mr. Brank[er], who brings with him Kinckhuysen's analytical conics turned into your method, which if it obtain your approbation may, I hope, be printed to the advantage of students, but withal I could wish he would enlarge it out of Bramer's Apollonius Cattus oder Geometrischen Wegwyser, made now into two parts in 4to, and reprinted at Cassel; the first part 1646, the second in 1647, which I am not without hope to procure. I conferred yesterday with Mr. Oldenburg, who promiseth to send his lordship Evelyn's Gardener's Almanac by itself. If I cannot procure it, he says Martyn and Allestrie printed it, of whom I shall endeavour to obtain it: he will also acquaint his Lordship with Col. Blount's proposals. My Lord may be pleased to have and keep Dettonville[c], and the second part of Galilæus, if they

[c] Lettres contenant quelques-unes de ses inventions de géometrie 4to. Paris, 1659: published under this assumed name by Pascal.

are liked: that part of Galilæus that is wanting was omitted by Leybourn in the gathering, and seems not to be very material, that argument being since handled by Torricellius, which Mr. Salusbury did not translate, and I may tell you of more he did not, to wit, the life for the most part was wrote by Mr. Bargett, an Oxford scholar, out of letters procured from Viviani in Italy; and Archimedes de insidentibus humido was translated and perchance enlarged by Mr. Rooke [d] deceased. There are few saved [e], and Hayes sells the first tome of the second part for 50*s*., an unconscionable rate, and wisheth he had more of them, he should not want customers. Mr. Branker hath a catalogue, which he promised to restore, of some books I cannot procure; but therein was omitted two or three, whereof I now send you the names. Dr. Croone hath many books he is minded to barter or put away: if they be such as his Lordship likes, when catalogues are exchanged a mutual barter may ensue. My humble service to his Lordship, who perchance may be absent, and causeth this address to yourself, whose perpetual happiness is the oraison of

Your devoted servitor,

J. C.

Leonardus Duardus de cambiis. Fol. Neap. 1641.
Vairus de variis fascini speciebus. 4to. Paris, 1582.
Fortunatus Licetus de centro et circ. 4to. Utini, 1640.

[d] The same who was mentioned by Bp. Ward in Letter XXVII. For an account of him see I. Ward's Lives of the Gresham Professors.

[e] This refers to Salusbury's Mathematical Collections, the latter part of which was almost entirely destroyed in the fire of London (1666), and therefore is now exceedingly scarce. There is a copy in Lord Macclesfield's library, which most probably was that which belonged to Collins.

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Bayfius de re navali, vestiaria et vas Rom. Paris, 1529.
Paduanus de horologiis. 4to. [Venet. 1582].
Guiffus de eclipsium prognos. et effectibus. 4to. Neap. 1621.
Crispus de ethnicis philosophis caute legendis. Fol. Romæ, 1594.
Cabeus de meteoris. 3 vol. Fol. Romæ, 1646.
Autolycus de sphæra, quæ movetur, cum Theod. Trip. de hab. 4to. Romæ, 1587.
De vario ortu et occasu ast. inerrantium. 4to. Romæ, 1588.
M. Psellus de op. dæmonum. 8vo. 1617.
Subtilissimæ calculationes Suiseth. Fol. Ven. 1520.
J. Caranta de auro arte facto. 4to. Savil, 1623.
Sam. Bocharti Geographia sacra, seu de dispersione gentium et terrarum divisione facta in ædificatione turris Babel. Fol. Cadomi, 1646.
Petri Beausardi usus annuli ast. 8vo. [Ant. 1553].
Jac. Capellus de pond. mens. nummis. 2 vol. 4to.
Maltherus de forma et quantitate anni diluviani qui fuit 600 N.
Claud. Polacci anti-Copernicus. [4to. Venet. 1644].
Hier. Sirturus de telescopio. [4to. Francof. 1618].
Inchoferi historia latinitatis. [8vo. Monachii, 1638].
J. de Laet de origine gentium American. cont. Hug. Grot. 8vo. [Amst. 1643].
Balth. Meisneri Philosophia sobria. 3 vol. 8vo. [Giessæ H. 1611, Witt. 1623].
Mythologici Latini, in quibus, Hygini Fulgentii, et aliorum fabulæ. 8vo.
Fortification nouvelle de Goldman du Freitach Comte Pagan du Ville. Fol. 1645.

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XLVII.

COLLINS TO ———.

About the printing of your comment the stationer, in regard of our present distractions, is not (at least as yet) willing to put it into the press. I communicated the same to Dr. Wallis, who was here last w[eek], who affirms he hath added nothing to the Clavis by reason of his opinion to the contrary: he was once spoke to about it, and mentioned a tract of his own of angular sections, which might have come properly in at the end; the Dr. having in haste perused your comment is indifferent about the printing of it here, but would not recommend it to the stationer at Oxford; saith the paper pinned on to the surds is erroneous, and would have it omitted; as to Figurate Arithmetic, it is largely handled in Maurolycus, the Opuscula of Bullialdus on Theon Smyrnæus: see also Guldinus, Faulhaber, Boethius, Jordanus; somewhat in Lantz, in Schooten's Miscellanies, Broscius, as also lately in Pascal du Triangle Arithmetique, but most fully and analytically in Floribus Alg.

The mention of the distraction, from which the publishers were suffering, seem to indicate the effect of the fire of London, in which, from the nature of their stock, they were very great sufferers. It is remarkable that there are two copies of Oughtred's Clavis in the Savilian library, which belonged to Wallis, and he has written a number of memoranda in each of them; but they both belong to the first edition of 1631, and he may therefore have communicated the substance of them to the author during his lifetime. From what will be found in a future letter about Dr. Rawlinson, it is most probable that this was addressed to him. Two other persons are also there spoken of as having drawn up comments on the Clavis.

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XLVIII.

COLLINS (MEMORANDA).

Mr. Branker [f], about

Dr. Thorndike; Pascal Comp.; sides and diag. trap.; Dr's. money; Descartes; to have title of Algebra; to inquire of the Doctor about Strauchius his Astrognosia; Jungius his works; where the Dr. saw Dioristice; to send back Hodierna's Archimede Redivivo, and another little 4to, answering Laurendarius problems, and this paper when transcribed; I send his music book and Evelyn's Gardener's Alm. and one of three Alms.; inform the Doctor I have caused Farby to be inquired after; title of his book.

Dec. 5. to Mr. Barrow with complem^t. of Navigation.

XLIX.

COLLINS TO MR. JOHN TEMPLER [g].

Besides the books mentioned I have seen,

Dettonville.
Beaugrand's Geostatice.
Ejusdem Algebra.
*Bartholinus in Opticam Heliod. Larissæi.
*Ludolph. a Ceulen de circulo et Ads. Sybrand Hanz Rud; á good table of segments.
Laloveræ El. Tetrag.
Hugenius de magnitudine circuli.
Ejusdem tractatulus de quadratura ex data fig. vel port. grav. centro.

* Scarce.

[f] Fellow of Exeter College, Oxford. See Wood's Athenæ Oxonienses.

[g] Described as being of Braybrooke; author of some papers in the early volumes of the Phil. Trans.

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Casimir's ars magna Art.
*Billii Diophantus Geometra et de prop. harmonica.
Peter Coursier.
De Guevara in Arist.
Baldus in spec. ust.
John Bapt. Benedict de omnibus problematibus Euclidenis.
Broxhius unica circini apertura perficiendis, of which see Cardan de subt.
Antimi Farbii opus.
*Zucchii optica mech.
*Gregorii optica.
Mengoli geometria speciosa.
Scheiner's rėfractiones.
Angeli.
Gerrard's Trigonometry.
Errard Barleduc's Solids and Henrion's, and Sph. Trig. pro.
Waldechii Welandi strena math.
Claramontius de Lunæ phasibus cum geometricis.
Cavalieri specchio ustorio.
Remelinus about fig. N. Faulhaber.
Ruota planetaria direct.
Vincentius Viviani de maximis et minimis. fol.
Jordanus de ponderositate.
Baliani de motu gravium.
Ghetald variorum prob. collectio.
Dibuadius în Euclidem.
Caravagius.
Anderson's Math.
Pellius contra Longomontanum.
Snellius Cy.; Ap. Batavus.
Torricellius de sphæra.
Goclenii isagoge optica.
Kepler's Stereometria.

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Moloheri prob. Deliacum.

Philolaus (i. e. Bullialdus) de vero systemate mundi, cum replicatione Deusingii.

Hecker's Eph.

Hevelius cometographia.

Vossius de origine Nili.

To know what Mr. Clarke doth with Sir Justinian Ison [Isham ?] and many others that are common.

*Bullialdus in Theonis Smyrnei m. et tract. de lineis spiralibus.

Borelli's Euclides restitutus.

Directorium Math. Abdiæ Trew.

Mariæ Cunitiæ U[rania] P[ropitia].

Mersenni.

De Sagarum natura et potestate deque his recte cognoscendis et puniendis physiologia. Marpurgi 1588.

Francisci Patricii Magia philosophica 1593.

L.

COLLINS TO DR. PELL.

Reverend Sir, April 9, 1667.

I have Mr. Branker's (accompanied with sheet X) wherein he seems not fully to understand my meaning, which, that it may not be mistaken, I shall here more fully enlarge. Being once in Mr. Thomson's shop, I met with a Cambridge scholar, who suggested that the small anonymous Jesuit's Euclid, printed by Mr. Martyn, was now in good request amongst tutors and their scholars there, that they wanted the like for arithmetic and algebra, that it was the sense of Mr. Jenkes and some others at Cambridge that Mr. Martyn should be prevailed with to print Strauchii compendium arithmeticæ or the Arith. and Algebra of Laurembergius,

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and Van Schooten's Principia Matheseos, which is not to be had alone. I replied, that Tacquet's Arith. was newly reprinted, but that they think too large, and his Algebra not to be expected till those other works of his now in the press at Antw., whereof I here send you the title, were dispatched, and that your book was now in the press. To which he replied, he had seen it at Mr. Pitt's shop, that the latter part of it was indeed very excellent, but the first sheets of it not so good an introduction as the Principia Matheseos, to which 'twas replied, that it was your scholar's, not so altered as to be owned by yourself, and possibly might be wrote by your scholar before Principia Matheseos was extant. I acquainted the gentleman that Kinckhuysen in low Dutch did write an introduction to Algebra in 1661, which had in it the doctrine of surd numbers, binomials and residuals and their roots, and more than either the Principia or yours. He told me that one Mr. Jefferies did agitate with Mr. Martyn about it, and I being acquainted with Mr. Jefferies procured him a sight of Kinckhuysen. The said Mr. Jefferies is upon going over into Holland with the ambassadors, and hath promised to procure it there and get it translated for Mr. Martyn, which Mr. Pitt is willing to prevent by having it himself, or by prevailing with yourself or Mr. Branker that some appendix about these matters may be further added to your book, to which I humbly suggest, that I think a Latin preface to explain the symbols, and to signify that the greatest part of the book may be understood by others, ignorant of our tongue, would be advantageous. Both these scholars think that division in species and the finding all the divisors, &c. may be much sooner and easier understood when a precept is worded than otherwise.

As for sheet V, after it was twice corrected, it was

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printed off, contrary to my expectation, without a third revise, and so some errata have escaped, as page : here the corrector took out more than I deleted, which I hope by a greater diligence to prevent hereafter.

Mr. Moore and some others have a high esteem for the notes of Huddenius at the end of Des Cartes, (which you have not read,) which are fully and very largely explained in your method by one Mr. Merry, in 30 sheets[h], who hath lent me his papers, and saith they may be reduced to three sheets, which I am willing hereafter to send Mr. Branker, so as to give the begetting equations, in lieu for the loan of his translation of Kinckhuysen's Conics, the which wanting your approbation shall not be printed by my advice or procurement, the rather because Varenius, in the preface of his most excellent history of Japan, saith thus[i].

You are now come nigh the 105 theorems about sines, tangents, and secants; and that I may not hide any thing from a person I so much reverence, I have this to suggest: Dr. Scarborough once lent me a thin folio, entitled, Problema Austriacum, dedicated to the emperor, but when or where printed not intimated, nor the author's name or letters of his name. The book I

h Wallis (Opera, vol. ii. p. 150.) mentions this tract of Merry being put into his hands by Collins. It is now in the Savilian Library; being clearly written on 235 4to pages, which answer to the 30 sheets mentioned in the text.

i The passage referred to in Varenius is as follows: Etenim quæ in conica, difficillima totius mathesis parte speculativa fueram, ea typographi excudere abnuebant, propterea quod ea studia paucissimorum hominum esse dicerent; atque ideo pauca valde exemplaria et non nisi multis annis divendi, ita ut tantum non lucrum ex illis facere possint, sed ne quidem expensas et sumptus in impressionem faciendos recipere.

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translated, and cannot now find my translation, and the doctor hath it not in town; wherefore I must trust to my memory in giving you the argument of it, which is this problem.

To find the side of a regular quatuordecangle inscribed in a circle. This he brings to an equation, which he solves numericè, and being a solid problem effects with the intersection of a circle and hyperbola, and saith if his method be prosecuted, in it lurks the mystery of angular sections; by it innumerable equations will be easily raised that will otherwise scarce ever come to light; a series of polygons will be given; the length of any arch belonging to a sine calculated easily in most vast numbers and the converse; and that the lengths of all arches fall in the convexity of the same hyperbola. Hence the table of sines, the areas of segments, and the division of a semicircle in a given ratio from any point in the semidiameter put into your power, of all which he hopes to treat more fully hereafter. Now in Francis Xavier Ainscombe's Vindication of Gregory's quadratures he honourably quotes a treatise, entitled, Elucidatio Geometrica Prob. Austraici, wrote by Gotefridus Aloysius Kinner de Louverthurn, but saith not when or where printed. I should be apt to conceive the author hath promised no more than he can perform, if the sines in a quadrant be so far lengthened without it till the sine and its extended part be equal in length to the arch to which it belongs, provided those extremities fall in an hyperbola, which possibly hath passed under your consideration, or at least may deserve it. Most easy and evident it is, if they be made equal in length to their chords they fall in a parabola.

Mr. Branker's letters are sent away and sheet V.

A bookseller tells me he shall [have] Sir Wm. Per-

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sall's Treatise of the loadstone and navigation to print, wherein he saith he will perform somewhat about the Merid. Line, concerning which you may see what Mercator saith in the Phil. Tr. N°. 13.

LI.

HOOKE TO OLDENBURG.

Sir,

I have not yet had any time to return M. Auz[out] an answer to his last printed return to what I formerly sent him, but in the mean time, according to your desire, I send you here a method, differing from any he has propounded, by which a glass of a small sphere, if plano-convex, may be made to refract the rays of light to a focus at a much greater distance than is usual. Prepare two glasses, the one exactly flat on both sides, the other flat on the one side and convex on the other, of what sphere you please; let the flat glass be a little broader than the other: then prepare a cell or ring of brass very exactly turned, into which these two glasses may be so fastened with cement that the plane surfaces of them may lie exactly parallel, and that the convex side of the plano-convex glass may lie inward, but so as not to touch the flat of the other glass. These being cemented into the ring very closely about the edges, by a small hole in the side of the brass ring or cell, fill the interposed space between these two with water, oil of turpentine, spirit of wine, saline liquors, &c. (then stop the hole with a screw,) and according to the differing refraction of the interposed liquors, so shall the focus of this compound glass be longer or shorter. This, when you have an opportunity, you may please to communicate, with my re-

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spects, to M. Auz[out]: and if he think fit to communicate his observations about the two last comets, I shall be very ready to return him an account of any thing of that kind, or of any other experiment which I have made.

The mention of Auzout's two comets probably refers to what was printed in the Phil. Trans. vol. I. pp. 3, 36.

This letter has neither signature nor address, but is in Hooke's handwriting, and the following memorandum is written on the back of the paper by Oldenburg.

So far the proposer of this method, who, in communicating it, does not so much maintain the practicableness of what is therein contained, as desire to give an instance (among many others) that some peculiar properties or qualities of natural things may enable those, that know them, to perform with ease such things, that to others seem either not feasible or not practical without great difficulty. But this I would have looked upon as one instance of many, (for there may be others,) of the possibility of making a glass, ground in a smaller sphere, to make a telescope of much greater length; though (not to raise expectation too high) I must add, that of spherical object-glasses those are the best, which are made of the greatest sphere, and whose substance has the greatest refraction.

LII.

BRANKER TO COLLINS.

Sir,

When I sent Kinckhuysen, I purposed to prevent it by a letter per post, but my disturbed affairs hindered me. I understand by yours to Dr. P[ell] that you have that book. I wish it may in any way pleasure

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you or any friend of yours. Only, I pray take care that it be not printed, either as it is or with any amendments, without my own, but especially Dr. Pell's, consent. For though he likes not the grounding of geometry on the cone, &c. yet he doth not slight the equations and processes therein, and makes me hope that his instructions (if God bless me with more of his company) may enable me to do somewhat upon the conic sections to as good purpose as that piece, and in a more methodical process. My wife was lately (Dec. 12) delivered of two daughters, one of which God took to a better life yesterday, (by an aposteme in her head, which I saw break out at her nostrils,) after she had lived a painful life here about fifteen or twenty minutes more than 365 hours, being born before 9 A. M. Dec. 12, and deceasing at 2 P. M. Dec. 27. I have not been very well composed in mind, you may guess, in the time of her sickness; but since God hath received her, I endeavour to say, as David, She cannot return to me, &c. I desire your prayers to enable the parents to understand and improve the afflictions. I pray excuse my haste. Dr. P. will say about the algebra what he thinks fit in his next to you. You may direct any letter to me either as before, (adding, to be left with Dr. Pell,) or else (to at Newchurch, near Vale Royal, to be left at Torperly in Cheshire. Stone bag,) for I now reside a while with my wife at this place. Either will bring safe [any letter].

My sincere respects &c. to Mr. Pitt and other friends.

Yours to serve you,

Newchurch, Dec. 28, 1667. T. Branker.

This letter is printed in the Gen. Dict. vol. X. p. 544.

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LIII.

PELL TO COLLINS.

Sir,

I thank you for the Latin paper, which you say was sent out of France, concerning biquadratic equations, of which I might be silent till the press bring us to p. 125 Rhonii. Yet in the interim, I say,

1. He quotes some book of Schooten, that hath a rule of Dr. Hudden to the same end. I have no such book, nor do I know that any man in this country hath it.

2. He saith, Omisi quomodo cognita quantitas y possit haberi, &c. Should it not be, quomodo cognita quantitate y, possit haberi &c.?

3. I cannot commend his choice of an example in numbers. To shew the usefulness of his new rule, he should have given an example in some biquadratic equation, which is resolvable into one cubical and one simple equation; and therein he should have shewn how, by his rule, he can separate the single equation from the cubical more easily than we can do by any former rules. But he hath given us an example in a biquadratic, that needs not his rule. For it doth most easily divide itself into two quadratics at the first sight. They must be very unskilful in such work, that do not see that $aaaa - aa - 2a - 1 = 0$ is $\begin{cases} aa - a - 1 = 0, \\ aa + a + 1 = 0, \end{cases}$ and that therefore this biquadratic hath not four possible roots, as some have, but that two of them are impossible, contained in $aa + a + 1 = 0$. The other two are,

$$A = \frac{1 + \sqrt{5}}{2} = +1.61803398 \text{ \&c.}$$

$$A = \frac{1 - \sqrt{5}}{2} = -0.61803398 \text{ \&c.}$$

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which may be found without the help of his cubical equation, or any other's.

4. If you meet with any more of those eight rules which he speaks of, I pray you get a copy of them. I may have time to tell you my opinion of them.

If D. Br. be gone from the countess's house, keep my letter till I send you a superscription for a cover; that it may be sent to his house in Nottinghamshire, by the post.

I thank you for your news. I knew not that our ambassadors were yet gone out of England; so ignorant they keep your servitor.

May 22.

The following paper is inclosed in this letter. It is in Collins's handwriting.

Regula nova deprimendi æquationes quatuor dimensionum ad tertium gradum.

Proposita qualibet æquatione quatuor dimensiones habente et secundo termino carente, puta $+a^4$, qa^2, ra, $s=0$, quærend[us] est valor hujus æquationis.

$$+y^3 \left\{\begin{matrix} \frac{+qq}{+2r} \\ \frac{-2s}{+r} \end{matrix}\right\} yy - \tfrac{1}{2}qy + \tfrac{1}{8}r = 0.$$

In qua q designat quantitatem cognitam tertii termini æquationis propositæ suis signis affectam, r vero quantitatem cognitam quarti termini etiam cum suis signis: at s ultimum terminum eodem prorsus modo, quo D. Huddenius utitur in sua regula pro eadem depressione æquationum quatuor dimensiones habentium ad tertium gradum.

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Exemplum in numeris.

Esto $+a^4-1a^2-2a-1=0$,

$$\text{fit } +y^3\left\{\begin{matrix}\frac{+1}{-4}\\ \frac{+2}{-2}\end{matrix}\right\}yy+\tfrac{1}{2}y-\tfrac{1}{4}=0.$$

Id est $+y^3-\frac{5}{4}yy+\frac{1}{2}y-\frac{1}{4}=0$, vel facto $+\frac{v}{4}=y$ oritur $+v^3-5v^2+8v-16=0$, et fit $v=4$, et consequenter $y=1$, quod arguit propositam æquationem e duabus planis compositam esse ex octo regulis, partim a me, partim ab aliis inventis. Septem primæ ab eodem inventionis fonte proficiscuntur; at hæc ultima, quam nunc profero, paulo reconditioris est originis, et in ea de industria omisi quomodo cognita y, possit haberi incognita æquationis propositæ quantitas a, ut habeant artis periti in quo se exerceant, si forte in hac investigatione aliquam operam impendere velint.

Dicta regula Huddenii legitur in commentariis Schootenii supra Geometriam Cartesii.

LIV.

COLLINS TO BRANKER.

Sir, June

In yours of May 29 you say the Doctor is not so meanly conceited of the introductory part as some, it seems, will needs be; To this I had not leisure the last time to make a full answer, but say I know none that account the Introduction a bad one, but divers that think it might have been more plain, and ought to have been more large than it is. This is the judgment of divers of the virtuosi and of some teachers of the mathematics here, who all love and honour the Doctor; and I hope I shall do no less as long as I live,

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albeit I am of their mind, nor do I endeavour to make others of the same opinion, but say to them the Doctor did not much concern himself therein, but lets it come out as his scholar left it; and I further say there is nothing in it but what I fully understood, upon reading of it at Coventry in 1665, and corrected the errata of it. Dr. Twysden, that hath studied (quodammodo) Algebra, as I believe, these twenty years, protests he cannot understand it. Mr. Martyn the bookseller will not follow the advice of Mr. Jenkes at Cambridge about printing any thing of that nature, having engaged to take off a competent number of the Clavis, which is reprinted as it was without alteration. What Dr. Wallis hath wrote will be an Appendix by itself; but this I have to say to second my last, that Mr. Kersey's Algebra is ready for the press, and he would not tell me, but I know it from others, that the wooden cuts for it are now in hand; and to speak home, Mr. Leake, Mr. Gunton, and others account it an excellent tract, his Introduction copious and easy; but he intends not to meddle with any solid problems, nor do I think there will be one problem in the Doctor's book and that alike, save that page 125, 126, Rhonii, about fitting in a line of a given magnitude between one side of a square and the other produced, that may belong to the opposite angle, which Kersey doth by a quadratic equation, and saith it may be doubted whether such geometric problems as amount to biquadrates, which may be divided into two quadratic equations, may not be resolved without surmounting quadratic equations.

What Mr. Kersey hath hitherto published hath been well esteemed; and this is his masterpiece, having been, I presume, ten or twelve years on the anvil; and I would not that his should either come out sooner

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than the Doctor's, or that his Introduction should have a better esteem. This moved me in my last, (which was the prænuncius to acquaint you that the new edition of the first and second volumes of Des Cartes is sent,) to write to you to incline the Doctor to admit the first seven sheets of the Introduction, enlarged out of Kinckhuysen, (which Mr. Kersey never saw, and the which I have now in my hands, and, upon your answer, am ready to send,) to come out as your translation, as soon as may be; the Doctor taking what time he pleaseth to supply the defect at the beginning, and to enlarge and complete the book. To which I have this further to add, that page 55 may be reprinted, having some error in it, and page 54, [where] the first edition of Van Schooten's Principia is quoted as erroneous, which you will see is amended in the second edition, whence 'tis evident the Doctor never knew of the two books now sent. But I wonder, when a good while since I wrote about them, the Doctor answered he [had] not read them. Whence it doth not follow that he had not heard of them or seen them, which if I had known sooner, he should not so long have wanted. I hope the Doctor will not be offended at my freedom, but rather like it, that I do not flatter him. Whereas the book hath many errata in it, I shall offer Mr. Pitt the loan of my servant (for his meat and drink) to correct it, and he writes a good hand.

News.

About the residue of Des Cartes's Letters; third vol.

In my last I mentioned a problem as plane, which I believe is solid, my papers about it being lent out.

The errata which are not such.

Mr. Price, at Speed's, at the Mermaid in Fleet-street.

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This letter is printed in the Gen. Dict. vol. X. p. 544. It refers to the Introduction to Algebra, translated from Rhonius out of the high Dutch by Thos. Branker, much altered and augmented by Dr. Pell. 4to. London, 1668. The 105 theorems, &c. mentioned by Collins (p. 127) were omitted.

John Kersey was born at Bodicot near Banbury in 1616, and was encouraged in the study of mathematics by the Denton family, of Hillesden, in the county of Bucks. He published his Algebra in fol. Lond. 1673, 4. It consisted of four books, the two first of which were republished by Halley. Aubrey says that he was employed as a surveyor, and that he died in London of a consumption before the year 1680. The "Weekly Memorials of the Ingenious" were published in London in 1683, by H. Faithorne and John Kersey; possibly a son.

LV.

TO MR. BALDROE[k].

Sir,

The advancement of natural and useful knowledge being the design of the Royal Society, whereof we are members, and understanding that you have opportunities before you of doing much service to the commonwealth of learning, we thought we should be wanting to that honourable design, should we be silent. We therefore suggest that we have often heard that the worthy and learned Mr. Barrow hath divers treatises in a good forwardness for the press, and some of us have lately seen his Treatise of Optics, which he prepared to deliver in to the former Vice-Chancellor,

[k] Described as Master of Christ's, and Vice-Chancellor. But no such name appears in the list of the Heads of that College. Dr. Baldero was Master of Jesus from 1663 to 1679, and Vice-Chancellor of Cambridge in 1668; but no one of either name appears to have been fellow of the Royal Society. That description, however, was probably intended only for the persons, who were to join in signing the paper, with Collins, by whom it was written.

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as his anniversary lectures, according to the laudable constitution or injunction laid upon your mathematic professor; but we fear the author's modesty is such that he will not promote the publication thereof, unless excited thereunto. Moreover it is well known that Commandinus upon the four books of Conics of Apollonius Pergæus is a book very dear, and exceeding scarce, and his text generally quoted; this book, we understand, is well explained and epitomized by the aforesaid Mr. Barrow: and not only so, but that he hath likewise done part of the three other books, published by Borellus at Florence. He wants time to finish that work, the which he might well accomplish, should he be by you desired to make the argument of those remaining books the subject of his lectures. A stationer here, that is a friend of his, is very desirous to be the undertaker, as is not unknown to the said Mr. Barrow.

We are induced to believe that length of time, and the persuasion of friends, may hereafter prevail with the said Mr. Barrow to publish some other good books by him intended, as his Comment on Archimedes, on the Spherics, his own Perspective, Projections, Elements of Plane Geometry; but those above mentioned seem at present to be in the greatest forwardness, and in other respects the sooner to be desired; and seeing England affords persons no whit inferior to the best of foreigners, we may presume we do not trespass in urging a common benefit to learning, wherein we cannot but promise ourselves your concurrence, which will very much oblige.

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LVI.

COLLINS TO VERNON.

Sir,

The learned and worthy Mr. Edward Bernard, fellow of St. John's College in Oxford, was lately in London [1], and he hath seen (by my means) an account, and desires one of a kind, of several new books lately published in France and Italy. I furnished him with some books which he had not seen before, and amongst the rest Pascal du Triangle Arithmétique, which with some others I received from M. du Hamel, the learned philosopher, to whom (when he was here) I gave a catalogue of some books, that I got his friends here to solicit him to procure. And for those he hath already sent, (and more I do not expect from him,) I am indebted to him four livres five sols, which Mr. Bernard wrote to you to pay to the said M. du Hamel. The said Mr. Bernard assured me that I might, by his means, promise myself your assistance and friendship as to such matters, that concern the advancement of learning and mathematics; and I have heard no less from our good friends Dr. Wallis, Dr. Wren, Mr. Hooke, Mr. Kersey, Mr. Gregory. And now, sir, I am to shew you wherein you may do us all a common kindness. One Mr. Hoote, the son of an eminent merchant, having been in France for recovering his health, (being in a deep consumption, whereof, by God's blessing, he is well recovered, and returned,) grew acquainted with the affable father John Bertit, a Jesuit, of Lyons, a learned man and

[1] After these words Collins had written but afterwards drew his pen through the beginning of them:—" And he was pleased to enter into an acquaintance with me, I presume by means of Dr. Wallis, who had shewed him some of my letters, in which I gave the Doctor an account of several new books, &c."

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excellent mathematician; and that begot a correspondence between him and me. I sent the said father some presents of books, and he intends the like to me. I wrote to him to procure many more, (which in vain I have formerly sent for to Paris,) and he promiseth to send all or most of them. Dr. Wallis wrote to him on my behalf, to assure him that he might expect candid upright dealing from me, and he intends to write back to Dr. Wallis. The said father Bertit is now at Paris, to be found in the house of M. Verjus, the king's secretary, and he is worthy of your acquaintance. He is indeed the true Mersennus of France, and hath given a notable account of books and learning, (which I communicated to Dr. Wallis and Mr. Barrow, &c.) That which I would request is, that you would vouchsafe to visit and confer with him, to acquaint him that his letter, bearing date the 15th of January, stilo novo, I received not till 4th February, stilo veteri, which was above twice as long a coming as I have had one formerly from him at Lyons: and this happens, because he sent it to M. Legendre, at Rouen, to be sent to Mr. George Cowart, a merchant at London, to be delivered to me; whereas it would have come much sooner if immediately delivered to the post, and directed to Mr. John Collins, at his house, next the sign of the Three Crowns in Bloomsbury Market, London. I hope, within a post or two, to give an answer to it. Another request is, that if you hear of a good opportunity how he may send a box of books to me, which he hath in readiness, that you would be pleased to give him advertisement thereof. The way I formerly wrote was to send the same to M. Thomas Legendre, an eminent merchant at Rouen, to be directed to Mr. George Cowart; but such a passage may prove longer than we could wish: and unless he finds an opportu-

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nity of sending them to M. Jolley, at the French ambassador's here, I know of no other conveniency. I wrote to him, the said father, that I kept the accounts of the alum farmers here, who continually have a good quantity of alum at Rouen, in the hands of M. Legendre, as also monies there, who would cause his factor at Paris to pay to M. Verjus, or whom the said father Bertit should appoint, such sums as he should want or disburse, to buy the books I desire, which I presume connot exceed sixty livres. He answers he will not yet have any monies, and that M. Dandy, at the house of M. Verjus, will disburse what is requisite: but, sir, if you would vouchsafe to take that trouble upon you, and reimburse them when it is necessary, I shall immediately cause M. Legendre to be wrote unto to order his factor at Paris [to] pay you what monies shall be necessary, or if you please I shall pay it to any person here whom you will appoint, for your use. And in regard I am desired and delight [to] furnish my friends before mentioned, and others of the Royal Society, (whereof I am a member,) with books they cannot meet with in stationers' shops; I have therefore occasion to use more of a kind than one: I wrote to father Bertit amongst the rest to procure Fermat's Diophantus, (which is newly come out,) which Mr. Kersey earnestly desires to see; the said father promiseth to send me a new Treatise of Dioptrics, published by a Capuchin [m], and I could wish he would send more than one. Sir, having loaded you with this trouble, I wish I could be anyways serviceable to you here, and if so, you would command none more freely.

7th Feb.

Francis Vernon, Esq. was a student of Christ Church, and in after-life became fellow of the Royal Society. In 1669 he

[m] Dioptrique Oculaire, by Cherubin d'Orleans, fol. Par. 1671.

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went to Paris as secretary to the English ambassador, and when resident in France kept up an active correspondence on philosophical subjects with the friends whom he had left in England. See Wood's Athenæ Oxon.

LVII.

FROM COLLINS FOR SLUSIUS.

Mr. Oldenburg.

Sir,

Pray return most hearty thanks to the learned and affable Mr. Slusius, and signify to him that I have sent to Mr. Daems, to be delivered to Mr. Elzivir, to be sent to him, Dr. Wallis his book De Motu, Mr. Barrow's Optics, likewise Mr. Gregory's Exercitationes Geometricæ, lest he should not have received the former; and that within six weeks I hope to send Dr. Wallis his Treatise de Calculo Centri Gravitatis, which will be about 60 sheets, and Mr. Barrow's Geometrical Lectures, wherein that learned author doth apply his method of tangents to great variety of curves and the powers of their ordinates, not heretofore treated of, in order to the quadrature of those figures and the cubature of their solids; and you may further add that as to myself I have but mean knowledge in these affairs, which I obtained, by my own studies, at some small spare time from public laborious employments as an accountant, and at present all the spare time I have is spent in correcting of Dr. Wallis or Mr. Barrow's books at the press, and in drawing the schemes; that I do believe Mr. Barrow and Dr. Wallis are so far tired with what they have in hand, that they will scarce consider some curves propounded to them, which though they are of great use, yet the old pro-

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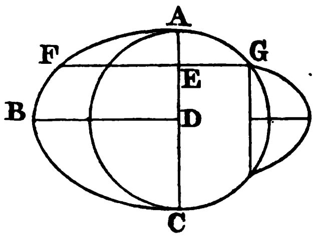
verb is verified, Difficilia quæ pulcra; and seeing he hath so candidly offered himself, let him pardon our presumption if we mention what we have propounded to them.

If AC, the axis of a sphere, be the base of a parabola, whose axis BD may be conceived to be one of the sides of a rectangle equal to the area of the circle, that begets the sphere, the other side being equal to the radius of the sphere, which may be put an unit, and that the parabola be divided by its diameters into trilinea and zones, the area thereof (multiplied by unit, which, to remove comparisons between a plane and a solid, may be conceived to be the height of a cylindric solid raised upon the parabola as a base) shall be equal to the respective segments or zones of the sphere. As for instance,



The area of the trilineum FEA = segment of the sphere FEG. [Sic in MS.]

And for the use of guagers I facilitated the calculating the segments of a sphere by these rules.

But if instead of the diameter of the sphere we take in the diameter of the circle, that is, the base of the segment, out of the former is derived the following rule.

But as to the matter in hand, it is hoped that the learned will further advance this kind of doctrine, by finding out the locus of the basis of that cylindric solid, that is equal, or throughout in like manner proportional, to the segment of a segment of a sphere, that

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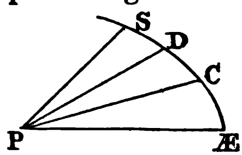
is, to the second segment of a sphere; a proposition of good use in guaging, for finding the content of a cask part out, below the head, which we would not have him trouble himself about, or at least not till he hath seen Dr. Wallis his book De Calculo Centri Gravitatis performing the same in another method.

About the drawing of tangents we may propose some spirals of good use in navigation.



Suppose the rays PÆ : PC : PD : PS to make equal angles at the pole P, and to decrease in continual proportion, that is, PÆ : PC : : PC : PD : : PD : PS, it is required to draw a touch line to any point in the said spiral.

This in navigation comes to this problem: a ship sails from the equator upon some rumb towards the poles; at last the latitude you are in and the difference of longitude is given, to find the rumb; if the eye be at the south pole projecting the rumb curve on a plane, touching the sphere at the north pole, it will beget the abovesaid spiral. This, in effect, is to require the straightening of the said spiral, and 'tis likely cannot be effected without finding of many continual means.

Suppose a sphere and a cylinder to have the same base, and that the sphere doth roll or evolve itself in the concave surface of the semicylinder, the section through the axis of the cylinder, on which the poles of the sphere move, to be horizontal: in this evolution the sphere may be conceived to leave its print behind it, in the semicylinder, which may fall flat, and it begets the common sea chart, with its meridians and parallels respectively parallel and equally divided. In this evolution the ecliptic, or any other great circle of greater or lesser obliquity, leaves its print behind

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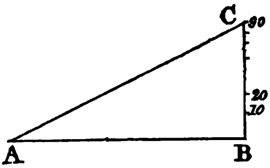
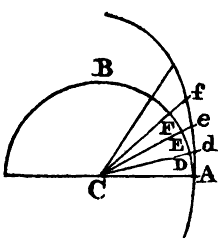
it, which, expanded in plano, begets a curve not yet treated of, whereof the ordinates, for example, for every 10° of arch of the equinoctial, are the rectified arcs of the tangents of those latitudes, through which the great circle doth pass, the which tangents are thus easily found.



Suppose AB to [be] the radius of the sphere, and that I would find the latitudes of the ecliptic for every tenth degree of [the] meridian, passing through the equinoctial; make BC, the tangent of 23° 30′, radius, and divide it as the line of sines, and draw lines from A through those sines, and they give the respective arcs sought, which being rectified, are the ordinates sought.

That which we intend to propose is a curve not yet treated of by geometers, serving for the rectifying of those arcs.



Suppose therefore in the circle CAB that the arcs AD : AE : AF, being rectified, are equal to the chords Ad, Ae, Af, in the spiral Adef, then will such a spiralis rectificatrix be of good use in the problem in hand, and the properties of such a curve deserve consideration. Or it may be said more generally, if the angles at C increase by equal differences, and the chords Ad : Ae : Af the like, the affections of the spiral deserve contemplation.

About the finding of the foci of the conic sections, I read something that seemed easy in Kircher's Ars Magna Lucis et Umbræ, quoted as taken out of Grienbergerus, but mistaken, which I improved as followeth.

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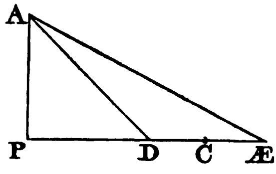

Let AP be the height of a pin on the horizon, and imagine the sun to have the same declination southwards as northwards, and that in winter-time he casts his meridian shadow to Æ, and in summer time to D, then if the latitude and declination be less than 90°, DÆ is the transverse axis of the opposite hyperbolas, and the half sum of the rays AD, AÆ reacheth from the centre C to either focus at f; but if the latitude and declination be more than 90°, the half difference of those rays reacheth from C, the centre of the ellipsis, to either focus, which after I had demonstrated and communicated to Mr. Gregory, he demonstrated more easily, and we here impart his demonstration; but think fit to mention one pretty problem.

Kinckhuysen, in his last Treatise of Algebra and Geometrical Problems, hath the problem about the foci, and treats of the method de maximis et minimis.

Mr. Barrow hath contrived three several systems or methods of curves for solving of equations, which he seems inclinable to print: and that the resolution of equations in numbers might be advanced, divers considerations have been proposed about resolving them by tables, whereof hereafter we intend to send you a specimen, as likewise about finding out the several series of roots belonging to such a series of homogenea, common to an equation, as have their least differences equal: of which affairs we doubt not his performance, and therefore entreat he will by his answers consent to enrich the republic of learning, admitting them to be printed in the Philosophical Transactions.

About the musical progression

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In answer to Riccioli's argument against the earth's motion, recite the account given by Mr. Gregory[n] in the Transactions, concerning the controversy between de Angelis, Riccioli, and others about it.

Also what is said of it in the account given of Tacquet's works[o].

De Angelis' assertion determining the spiral described by the compound motion of a heavy body falling to the centre of the earth was lately gainsaid by Ricci; and one Zerilli of Naples wrote in defence of Ricci's sentence against de Angelis.

LVIII.

TO SLUSIUS[p].

Reverend Sir, October

Both your letters, to wit, of the date of are received, with the inclosed papers, the one of your solution of some of the most difficult problems in the Optics of Mr. Barrow, the other in answer to a scruple by him raised about your manner of finding the arch or angle of a rainbow without tables; both which papers being communicated to him, he is well satisfied therewith, and commends the argute performances therein. He commenced Doctor of Divinity this summer [1670], in order to his being one of the King's Chaplains, and hath been lately in waiting. Hereby we intimate his laying aside of mathematical studies; but however find him not averse to communicate what he hath further done. One of the most difficult problems in his Optics, de loco imaginis in speculo spherico concavo aut convexo, was solved by Monsieur Hugens before his own

[n] Phil. Trans. vol. II. p. 693. [o] Phil. Trans. vol. II. p. 869.
[p] In Collins's handwriting.

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sickness and the publication of Mr. Barrow's book, and sent over only as a specimen of his new manner of printing, whereof we here send you a copy. One of our stationers here is very desirous to print in English a Treatise of Perspective, Catoptrics and Dioptrics: we would willingly encourage the design, having little or nothing of worth of that kind yet in English. Mr. Barrow is willing to communicate his Lectures of Perspective, which he read in English, but is not willing to own them as an author: to these we are desirous to annex Catoptrics and Dioptrics, and therefore crave your opinion and information concerning some authors, and which you like best. How do you approve Tacquet's Perspective and Catoptrics, or that century of Optic Problems of Eschinardus? is the whole century yet extant? what hath Manzini wrote of this argument? Schonbergeri Centuria Problematum Opticorum we have only heard of nomine tenus. Wentzel Jamitzer is said to have wrote of Optics, the compendium whereof is called Syntagma Optices, but we know nothing more but the name. M. de Beaune, in a short catalogue of good mathematical French authors, at the end of Irlon's French Grammar, is said to be mentioned as one that hath wrote of Optics, and that his Treatise thereof is extant; and from Paris they write it is so scarce it cannot be procured. We thought a translation of Dr. Barrow's book somewhat too difficult for the vulgar, and therefore trouble you with these interrogatories. We hope you have received the two books Mr. Collins last sent you by skipper Hanz, to wit, Dr. Wallis de Calculo Centri Gravitatis, and Dr. Barrow's Lectiones Geometricæ, on which book, according to your accustomed candour, we may promise ourselves to receive your learned observations, the which shall be communicated to the Society at

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their next sitting, and entered in their Journal, that none other may pretend title thereto, which we have little reason to suspect. Your animadversions on the author's last lecture about equations will be very welcome, and, seeing the learned Ricci desists, we could earnestly wish you would publish the application of the theorem in your Mesolabe (page 116), that completes the doctrine of determinations, to wit, your fourth chapter of the Miscellanies, treating de maximis et minimis.

Gerard Kinckhuysen, before his death, published three tracts of Algebra in low Dutch, viz. an Introduction, Conical Sections, and a tract of Geometrical Problems, solved by algebra, wherein he treats somewhat of the method de maximis et minimis. A Century of problems and their determinations, being formerly promised by Honorato Fabri, is lately published at Lyon, but we do not expect to find in either of these authors that which the learned are to hope from your pen; seeing we cannot expect it from one here, (who is incommunicative,) who hath often asserted to his familiars, that he can most exactly, any equation being proposed, shew what the homogeneum comparationis must be to make any pair or pairs of roots to gain or lose their possibility; and secondly, that out of that doctrine of limits he could fill up (with no great toil) columns containing all those ranks of roots, both negative and affirmative. We send you a specimen hereof in cubic equations, that our meaning may be the better understood. His way of doing it was not by depression, as here, but scandendo, the like in his limits, limiting precisely, first quadratic equations, then cubics, then biquadratics, &c.; then by aid of those columns making the roots ordinates, applied either to the respective homogenea or to the roots of

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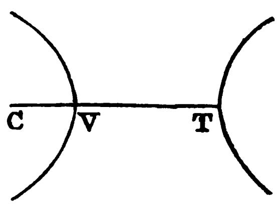
those homogenea, according to the degree of the first term of the equation, find the genus of curves proper to equations of each kind passing through the tops of those ordinates.

The describing of some of Mr. Barrow's curves by points, by aid of a series of rational numbers, depends on the solution of this problem.

In a rank of continual proportionals giving or assuming ad libitum the sum or the difference of the extremes, to find the terms of the said rank rational: if the number of terms in the said rank be but three, then in effect it is no other than to find the ordinates of a circle or equilateral hyperbola rational.

In the circle the diameter is the sum of the extremes, and there the problem results to this. To divide the square of the radius into two squares; which problem of Diophantus is likewise solved in Herigone, and from the solution a series might be desired.



In the equilateral hyperbola, if VT, the transverse axis, be an unit, then if the portion of the axis from V towards C be any one of the terms of the series following, the ordinate shall be rational; $\frac{1}{3}$ $\frac{4}{5}$ $\frac{9}{7}$ $\frac{16}{9}$ $\frac{25}{11}$; for example, $1\frac{1}{3} \times \frac{1}{3} = \frac{4}{9}$, the square root of which $= \frac{2}{3}$: also $1\frac{4}{5} \times \frac{4}{5} = \frac{36}{25}$, the square root $= \frac{6}{5} = 1\frac{1}{5}$. And the fractions may be taken away by making a fact of their denominators.

This problem deserves the pains of a commentator on Diophantus.

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LIX.

COLLINS TO VERNON.

Sir,

Yours of the 4th instant S. N. is received; the enclosed of P. Bertit is sent to Dr. Wallis.

I hope you have received mine of the 23d of November, which acquainted you with the receipt of the packet of books from P. Bertit, and what [was] contained therein, as likewise that your own packet was by me received, and left at your brother Welbie's. The Royal Society would needs have Cherubin's Dioptrics for their library. The mathematical intelligence in your letter was read at the Society; where another letter concerning the designs of P. Pardies was likewise read, intimating that he intended to print a second treatise of Collision and Local Motion, as additional to his former, and that he would publish the Cœlestial Globe in six charts, but should, out of the longitudes and latitudes of the fixed stars, be necessitated to calculate their declinations and right ascensions, the which is already done in Forster's Miscellanies or Posthuma, and in a late book of Navigation, published by Mons. Denys at Dieppe, in French. Forster's book is in folio, in Latin and English, and what errors have been found in those calculations I believe have been observed by Mr. Moxon and Mr. Flamsteed of Derby, who, when he knows the design, may be aiding thereto.

As to the papers of Dr. Rawlinson, deceased, I have this to say, I knew the author too well to believe that any thing of his is worth the printing, and I find Bishop Ward and Dr. Pell to be of my mind. I shall give you one of many reasons. Being in discourse

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with him about his comment on Oughtred's Clavis, I asked by what mediums he solved those four problems of the nineteenth chapter; to wit, in an oblique plane triangle, if there be given the base, (or difference of the segments of the base, which is likewise a base,) the perpendicular, and sum (or difference) of the sides to find the triangle; he answered by four several tedious analytical calculations, which he shewed me. I then told him they (and many others) might be solved by aid of this proportion; as four squares of the base are to the difference of the squares of the base and of the sum of the legs, so is the difference of the squares of the base and of the difference of the legs to the square of the perpendicular; provided, when occasion required, he made the middle terms extremes. This proportion I told him was easily derived from the third axiom of plane triangles; (and that is, As the base is to the sum of the sides, so is the difference of the sides to the difference of the segments of the base made by the perpendicular;) and the third axiom was easily derived from the 47th of the first of Euclid. And when I spoke with him again he discovered his ignorance so far, that he could neither derive the former proposition from the latter, nor the latter from the 47th of the 1st.

One Isles, a bookseller in Duck [Lane?], bought some of his books; and Anderson, a weaver, in company of Mr. Streete, bought more of them; and they have seen some of his writings, for which a great rate was demanded; and if I meet Streete accidentally, I shall with no great appetite inquire where they are. He is about to publish planetary instruments, and hath an exact theory of the moon; but this he will not publish without a reward.

As to Harriot, he was so learned, saith Dr. Pell,

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that had he published all he knew in algebra, he would have left little of the chief mysteries of that art unhandled. His papers [n] fell into the hands of sir Thomas Aylesbury, who was father to the late Lord Chancellor's lady, by which means they fell into the Lord Chancellor's hands, to whom application was made by the members of the Royal Society to obtain them: his lordship (then in the height of his dignity and employments) gave order for a search to be made, and in result the answer was, they could not be found. I am afraid the search was but perfunctory, and that, if his lordship (now at leisure) were solicited for them, he might write to his son the Lord Cornbury to make a diligent search for them. One Mr. Protheroe, in Wales, was executor to Mr. Harriot, and from him the Lord Vaughan, the Earl of Carbery's son, received more than a quire of Mr. Harriot's Analytics. The Lord Brounker has about two sheets of Harriot de Motu et Collisione Corporum, and more of his I know not of: there is nothing of Harriot's extant but that piece which Mons. Garibal hath.

One Bunning, near Coventry, hath wrote a Comment on Oughtred's Clavis; much of it taken from the Algebra of Scheubelius: and Clerke, mentioned in your letter, hath likewise writ a Comment on the said Cla-

[n] Harriot's will is not to be found, but Camden says that he left his property to Viscount Lisle and sir Thos. Aylesbury. Lord Lisle's share of the papers appear to have been given up to his father-in-law, Henry earl of Northumberland, who had been Harriot's munificent patron, and they descended with the family property to the E. of Egremont, by whom a large portion has been given to the British Museum, and the remainder are still preserved at Petworth. Sir Thos. Aylesbury's share became the property of his son-in-law Lord Chancellor Clarendon, to whom the Royal Society applied, but, as it appears, without obtaining them. (See Birch, Hist. R. S. vol. II. pp. 120. 126. 309.) None of these papers are now in the possession of the Earl of Clarendon or Lord Montagu, who are the representatives of the family.

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vis; but I have no appetite to recommend either the one or the other to the press, knowing what Mr. Kersey hath writ to be much better than either.

P. Bertit wrote me word, that he had caused M. Hozanna [o] to be sent for from Lyon to Paris to write a good body of specious algebra. I answered, that a much better than any extant might be derived out of these Dutch authors, to wit, Stampioen, Smyter, Faulhaber's Academia Algebræ, Martin van Wilkin's Officina Algebræ, Franc. vander Huyp's Algebra, Wouter Verstap of Figurate Arithmetic, J. Jac. Ferguson's Labyrinthus Algebræ, and three good books of Gerard Kinckhuysen, to wit, his Introduction, his Analytical Conics, his tract de Maximis et Minimis and of Geometrical Problems solved by an Analytical Calculus, with delineations thereto, to which many things may be added out of Slusius his late excellent Merolabe, Bartholini Dioristice sive Methodus æquationum prima et secunda, and his Treatise de Inventione omnium Problematum proportionalium, maxime harmonicorum. The said Bartholinus, Hudden, and Ricci have promised general bodies of algebra, but none of them, for aught I can understand, are mindful of performance. The Lord of Carcavi hath, it seems, a design to cause Diophantus to be turned into specious algebra. This [is] already done (but not printed) here by Mr. Kersey and Dr. Pell, as likewise by his scholar Rhonius, at Berne in Switzerland, a copy of a letter from whom to Mr. Haak, that translated the Dutch Annotations on the Bible into English, I here send you.

You may ask, what can England add to this purpose? I answer, Mr. Merry hath wrote a Comment on Hudden's Rules at the end of Des Cartes; one Dr. Davenant,

o [Ozanam ?]

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in Wiltshire, the son of the late Bishop Davenant, of Salisbury, hath spent the greatest part of his life in algebra, and wrote above two reams of that argument; Dr. Wallis hath wrote many things of algebra, which he would have printed, with his former works, in Holland, (seeing he cannot get it done here,) but I think they will not comply. To these I could add some few things that might be considerable: the adding of a musical progression, viz. the sum of such a rank as this, $\frac{10000}{106}, \frac{10000}{112}, \frac{10000}{118}, \frac{10000}{124}$. The sum of an hundred terms in such a rank is 3200, with an inconsiderable fraction: hence if an annuity of £100 per annum, to continue an hundred years, is to be purchased upon a discount of 6 per cent. per annum simple interest, the present worth will be £3200, which shews the absurdity of simple interest. The first year's worth is found by this analogy: If £106, due a year hence, be worth £100 ready money, what is £100, due a year hence, worth in ready money? The answer is, $\frac{10000}{106}$, and so of the rest. By this proposition you may give the area of the hyperbola, and raise the logarithms.

A rank of numbers, whose last differences are equal, may be many ways raised. I shall mention two, the one by adding the pure powers of any arithmetical progression together, or by taking the products of the terms of three or more arithmetical progressions any way placed.

			Products.	Diff. 1mæ.	2dæ.	3æ.
1	3	13	39			
2	5	10	100	+61		
3	7	7	147	+47	−14	
4	9	4	144	− 3	−50	36
5	11	1	55	−89	−86	36
A	B	C	P			

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As the rank P are the products of the terms of the progressions A, B, C, and the third differences are equal, I say that any rank of any degree whose last differences are equal, as the rank P, may have a common equation found to belong thereto, and to some one arithmetical progression as a series of roots, (and consequently applicable to any other arithmetical progression,) so that conceiving any possible number whatsoever to be given in one of those ranks, as C, the correspondent in the other may be easily found by aid of the said equation, which mightily facilitates the making of tables; but to give a number in the rank P, and require the correspondent in the rank C, is done by finding the root of an equation (in this case affected, and of the third degree or cubical). Hence you may conceive that all manner of tables solve equations, that is, such as are peculiar thereto. On the contrary, is it possible that some few tables shall solve or find the roots of all equations? Dr. Pell affirms it, and that he hath for thirty years past forsaken Vieta's method, and solved equations by the table of logarithms, &c., and saith he intends to write a treatise of this argument. Of all parts of algebra, this seems most worthy of consideration of the learned, and I have most urged it, by reason of his concealing of it, by communicating discourses to shew the possibility of it in many cases, and the probability of it in all. The wonderful ease ensuing thereby, none doubts, all desire.

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LX.

BERTIT TO COLLINS.

Sir,

I having found at Pontoise a good conveniency of an English gentleman called Mr. Floyd, belonging to my Lady Duchess of Cleveland, I would not fail to present you my humble respects, and to let you know that I have given to M. Justel three books, to wit, the Fabri's Geometry, and Dialogues of the same author, with a Capuchin's Dioptric. I have not received any of those books which you sent me by Mr. Vernon. I do expect shortly a great number of Father Fabri's works, which I will not fail to send you. It will be necessary that Mr. Flamsteed should write to M. Mouton, who will make the observations of the appulses of the moon unto the stars. The mathematical works of Caramuel Lobkovitz will cost four Spanish pistoles, which are in three volumes in fol.: and likewise Geometra Promotus of Carolus Renaldinus, which is of three volumes in fol. costs 12 crowns of Rome. The works of Petrus Mengolus, 15 Roman crowns, and are 4 vol. fol. There is a great rumour in the University of Paris concerning Cartesius's doctrine, which they would condemn as being contrary to the mystery of the Eucharist; but our faith may be explicated according to the principles of every philosophy. You will find here inclosed a letter of M. Regnault, in which he offers his observations and hypothesis of the moon to Mr. Flamsteed, and discovers his paralogisms about the amplification of visual objects. I made [or caused to be] copied my discourse of the rhomboidal stone of Iceland, to send you, with a good quantity of the same stones, which are found upon the Alps near Embrun.

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When I shall be at Paris, I shall take out two Spanish books of Algebra and Trigonometry of our Professor of [the] Imperial College at Madrid to send you. It is wonderful that a Spaniard should write a mathematical book. This author promised me many curious observations for your Philosophical Transactions. I wish you all happiness, and am

Your most humble servant to command,

At Pontoise, 26 Jan. S. N. 1671. JOHN BERTIT.

LXI.

DR. E. BERNARD TO COLLINS.

Honoured Sir,

I was too happy, a while since, receiving a letter from yourself, and another from Mr. Vernon, both which too much oblige by love and learning. I see I am altogether mistaken by you both, and a short commerce is sufficient to convince you of an error and haste. The mathematics I own, are but just enough to admire Dr. Wallis here, and yourself and some few others abroad: and the remain[der] of my study is literal, and so beside the fame and regard of this age and inferior, in the nature of the thing, if I may speak as fits the schools, to real learning. I must profess that there is [a] great deal of difference, as I esteem, between what is notional only and what is also useful, between the derivation of a word and the solution of a problem. Books and experiments do well together, but separately they betray an imperfection, for the illiterate is anticipated unwittingly by the labours of the ancients, and the man of authors deceived by story instead of science. The happy Royal Society adjusts both together, and I doubt not but, in a short while,

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will approve itself so great a friend and near ally to the Universities, that, by the munificence of some of the members of the noble fellowship, there may be occasion given of frequent experiments in both famous Universities, and consequently of a lasting commerce. It is but the hum[our] of a few, and those not much the desire and glory of either society, to [join] speculative knowledge to the practical, and the education of youth to the divertis[ements] of men. Though indeed the same person seldom excels in sermocination and mechanics together, yet nothing hinders but it may be the joint renown of a society: and this I must always affirm for the honour of my mother the University of Oxford, that if her children had the good utensils, which adorn the colleges of the Jesuits abroad, the world would not long want good proof of their ingenuity. Patrons and tools are rather wanting than willing and fit workmen. We lack a corporation, a set of grinders of glasses, instrument-makers, operators, and the like, that experiments may be well managed in this place, which otherwise, by reason of our living all, as it were, together, and our freedom from the intricacies and vexations of the world, is most convenient for such a design. This I say, lest amongst others you should have any expectation from me, the meanest member of the University, of aught conducing to natural knowledge. My delight is to be ascertained from yourself, the very Mersennus and intelligence of this age, concerning the effects of the great spirits in England and abroad, amongst which I thank you for the advice of the two Archimedeses, Borellus and Faber, which I would gladly see. I send you the piece of the honourable Esquire Boyle, De Origine Formarum, which I entreat you to accept as a small acknowledgment of my respects to you. I design a specimen of

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ancient mathematicians, as they are to be found in MSS., and also of what the moderns have left unpublished. I know you can well direct and furnish me herein, and therefore beg your favour. I would willingly know what books of this kind are in Arundel house, [or] in the library of the Royal Society which you adorn. This specimen will in part answer Mr. Streete's desire. Pray take my hearty respects and thanks yourself, and recommend them also to Mr. Oldenburg, Mr. Haak, to whom I am indebted for a letter and the good bearer, to Mr. Hooke, and good Dr. Jeffreys, &c. I am your very obliged

And ever loving friend,

April 3, 1671. E. BERNARD.

This letter is printed in the Gen. Dict. vol. III. p. 248.

Borelli published in 1661 the Liber Assumptorum of Archimedes, which, having been translated from the Arabic, was the more interesting to Bernard, who was eminent as an Oriental scholar. His own copy of this work, with many manuscript notes, is among his books in the Bodleian Library. He drew up Veterum Mathematicorum Græcorum, Latinorum et Arabum Synopsis, which was printed at the end of his Life by Dr. Smith. He did not become a Fellow of the Royal Society until 1673.

Honorato Fabri was a Jesuit, which probably occasioned the allusion made by Bernard to that order. Further mention of him, with respect to Archimedes, will be found in a future letter.

LXII.

COLLINS TO VERNON.

Worthy Sir,

I have yours of the 4th of April S. N. and am exceedingly obliged to you for the great trouble you are pleased to undertake at the request of a stranger.

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Having not received any direction from Pere Bertit how I should direct my letters to him at Paris, I have been the more dilatory in answering his, the rather till I hear whether he hath mine of the 21st of Febr., which, consisting of three whole sheets of paper, (besides Flamsteed's calculations of the moon's appulses to divers fixed stars for this present year,) gave him an ample account of mathematical affairs and books designed for the press here; and if it be miscarried, 'twill be matter enough for two or three letters to repeat. Concerning philosophical affairs I can give but a slender account, as having been absent from the weekly meetings of the Royal Society on Thursdays in the afternoons, at which time my attendance was required at the Council of Plantations, who sat at the same time, by reason of their attendance as members of parliament in the mornings; and I hear of no philosophical book in the press, besides a Treatise of Dr. Willis [n] of such creatures as live either under ground or [in] water. Dr. Wallis his former works are to be reprinted at Leyden, some of our booksellers joining with the Dutch in the impression. And in regard Bullialdus is printing somewhat against the Doctor's Arithmetica Infinitorum, I wrote to Pere Bertit to give an account of it, and when it would be extant; to which I now add, that the Doctor is unwilling the Dutch should reprint his Tracts de Motu, Libra, et Calculo Centri Gravitatis, lately printed here, nor his Mechanics, now in the press, which will be finished by next term, and therefore offers to supply as much in bulk concerning other arguments; and truly I could wish the Doctor had a sight of the Music and Mechanics of Des Cartes, lately printed at Paris with the

[n] De Anima Brutorum, 4to. Oxon. 1672.

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notes of P. Poisson, whichof I wrote to P. Bertit to send some. Mr. Kersey being almost ready to publish his Algebra, it is also desirable he should peruse Fermat's Diophantus and Barruel's Algebra gallice, which I hope P. Bertit will also procure and send; we should also desire Renaldini Geometra Promotus, but that he gives it cold commendations. I wrote to P. Bertit that a stationer here (to wit, Mr. Martyn, jointly with Mr. Faithorne, the graver) was desirous to print a volume of Optics in English, and therefore desired not only his advice, but that he would inquire for Mons. de Beaune's Dioptrics; such a book was promised, as appears in the Preface of Schooten's book De organica Conicarum Sectionum descriptione; and Mr. Jonas Moore, junior, being at Paris, saith he read an account of good mathematical books printed in France, at the end of a French grammar, which he thinks was Irlon's Grammar, in which the Optics of de Beaune were mentioned to be extant; and Mr. Oldenburg, at my request, (but, according to his custom, for his own disposal,) writing for the same, had for answer that it could not be procured. In mine of the 21st of February, I advertised P. Bertit that his Majesty's Ambassador at Venice had wrote word that these books were lately come out in Italy, viz.

Borellius de Liquidis,
Mengoli Opus Musicum,
Gottigniez Dioptrics, and
The Commentaries of Honorato Fabri on Archimedes;

and desired him to be instrumental to procure them all—the Dioptrics, for the reason above mentioned, and because I take Gottigniez to be a good geometer. The music for the perusal of the Lord Brereton and Mr. Birchinshaw, who labours in the composing of a

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body of music; and I myself have been a practitioner on the viol de gamba.

We are here so unhappy that we cannot get books that are common to be had in Paris, as Laloveræ Treatise de Geometria Veterum promota in septem de Cycloide libris, and Leotaud's last book entitled Cyclomathia, which books were extant six years before Dr. Wallis heard of either, though he was not a little concerned in both. At length, by my means, there was, with trouble enough, one of each kind procured for him: and I think there are no more in England. And when I tell you that divers eminent persons here desire me to procure them Fermat's Diophantus, you will see that to buy these books and pay P. Bertit for such books as he hath or will send, the money you have will soon be disposed of, which I humbly entreat may be done, and I shall cause another supply. The Society desire Mr. Flamsteed, of Derby, a young man, aged twenty-one years, eminent for his skill and pains in astronomy, should correspond with some good astronomer remote, as M. Mouton, of Lyon, who should be desired to observe the appulses of the moon before mentioned. This I signified to P. Bertit, who first of all wrote concerning M. Mouton that he earnestly desired that his works should be known in England, [that he] would send some of his Treatises of Astronomy to disperse here, and that he was a most sedulous observator. I could, therefore, wish that M. Mouton's books had been sent, which should either have been paid for or requited from hence.

You see that P. Bertit writes for a collection of all the Philosophical Transactions, in English, which, being about sixty-seven in number, will cost forty shillings in quires; and the stationer informs me that he doubts he cannot make above one complete collection

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of them in all. The said Pere also writes for an Etymologicon Linguæ Anglicanæ: this wants further explication. We have Cotgrave's Dictionary, in fol., the French before the English and the English before the French, price 14*s.*; and Goldman's excellent Dictionary, the English before the Latin and the Latin before the English, in 4to, price 14*s.*; and Skinner's new Etymologicon Linguæ Anglicanæ, in fol., shewing from what languages most of our English words are derived, price 25*s.*; but this I conceive is not so fit for his use. Moreover there are divers books in Latin and English, or French and English, besides grammars and school-books, and which of these he will have, I shall, as soon as I have an answer, comply, God willing, with his desires, in sending the same, with Evelyn's Sylva and Pomona, &c., and for the future give and take au account of what is disbursed on either side, and doubt not but in a little while to find out how to send to him and receive from him the Transactions mutually, without putting him or myself to the charge of postage. Pray present my service to him, thank him for his letters, and inform him that I hope ere long to write to him, at least craving your conveyance, if for want of information I know not how to shun putting you to that trouble. Mr. Oldenburg saith he had received the first sheet of a Latin impression of our Philosophical Transactions, begun at Hamburg; but it was so ill rendered, that he discouraged them from proceeding: father Bertit writes that they are translated into French, and that he hath read them; but by reason of his inquiries after a translation, I conceive they are not printed. I have been too troublesome already, and therefore, for conclusion, will only further add that I am

4th April, 1671.

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8th. Mr. Evelyn gives Sylva and Pomona.

		£.	s.	d.
Cotgrave	- - - - - -	0	14	0
Boyle de Stylo	[Considerations touching the Style of the Holy Scriptures, 8°. Lond. 1661.	0	1	6
Origo	Origin of Forms and Qualities, according to the Corpuscular Philosophy, 4°. Oxf. 1666.]	0	2	6
Experiments	. - - -	0	0	6
Transaction	- - - - -	0	1	0
		0	19	6

No Ven[a]. Transaction or chargeable packet desired by the post.

LXIII.

J. A. BORELLI TO COLLINS.

Nescio an adhuc ad vos pervenerint undeviginti exemplaria mei operis De Motionibus naturalibus a gravitate pendentibus, quæ una cum aliquibus aliis libellis transmisi. Modo intra arculam ad D. Henricum Oldenburg R. Soc. Anglicanæ secretarium missam inclusi, ut tibi tradantur, quatuordecim exemplaria mei opusculi de Ætnæ incendio, una cum opere posthumo Grimaldi de Lumine, et duas epistolas Fr. Redi de sede veneni viperæ, et libello inepto de eodem incendio. Amici Pisis collegerunt hos libros, novam de machinis philosophiam Nicolai Zucchii, centuriam prob. optic. Eschinardi duplicem, ejusdem centuriam aliam novam pariter duplicem. Iisdem jussi ut ad te eos transmitterent, et deinceps alios conquirant. Algebram speciosam Renaldini adhuc non vidi; sed audio nil novi continere præter ea, quæ ab aliis et præcipue a Michaele Angelo Riccio collegit, de quibus vos certiores

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faciam. Clarissimum Wallisium impense salutes rogo, eique mitte aliqua exemplaria mei opusculi de incendio Ætnæo, ut sibi et amicis eruditis sufficiant. Fama retulit (utinam falso) clarissimum Boylium e vivis migrasse, deplorabili totius literariæ reipublicæ damno. Audivi pariter Latine translatum fuisse ejus librum de origine formarum, obsecro ut eum transmittas una cum aliis libris de novo apud vos Latine editis, nedum scientificis, sed etiam historicis, aut politicis, exceptis iis qui de religione ex professo agunt. Tu interea, vir præstantissime, me amare perge, et vale.

Messanæ, 10 Aprilis, 1671.

LXIV.

BRANKER TO COLLINS.

Sir,

I received your letter in its season, but have deferred thus long to answer it, that I might be enabled to say somewhat that might answer your present desires. For I expected ere this to have received as much of what was due to me at Lady-day last, as might have discharged your debt and more. But it is not yet come to my hands, and I have not yet importuned my debtors for it. When I desired your forbearance until Lady-day I did not depend on that which is thus slack; but my occasions rising on my hands such as were not expected, I made bold with what was intended for you, being enforced to it, hoping that the residue, which I just now said was behind, might suffice. The truth is, I have lent Dr. Pell's children, here in the country, some money at his desire, not without sufficient grounds to presume on speedy repayment, nor indeed will my great obligations to that eminent person permit me to be backward in serving him, as I

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shall be able, nor do I believe you have any reason to distrust his integrity. But I hear of no payment yet. My salary for my [o]Lord's three sons and their man is £46 yearly, of which, although the first quarter be not yet paid, yet I do not doubt of it, having my Lady's own engagement to see me paid out of her own annuity, which is not always paid to her Ladyship at the day, but is very sure. Besides those four, I have four more scholars, which in all bring me in at the rate of £78 per ann. Which I tell you, that you may not fear my insufficiency, if your occasions can grant me time to procure what is from them my due, and every penny as sure as if I had it in my hand. I can (and will if you insist on it) take up upon use so much, as will reimburse you fully and when you please. But I am sorry your debtors at London are so slack. As soon as I receive it anywhence, you shall have it presently returned. But for Mr. Pitt, I know not whether I can oblige him so far as to move him in it to help me. The book which I am doing for him I can finish, if he grant me a little time. But he is in such haste as almost discourages me to go on. Neither Mr. Pitt nor any one else shall make me over hasty. I know not what acceptance your occasions will permit you to allow to these lines. All I can now say is, that I shall with all speed endeavour, together with your money, to shew my thankfulness for your great kindness, and this unexpected trouble, which you are put to for my sake. And when I shall have seemed to discharge all, yet still remain

Your ever thankful friend and servant,

Tilston, May 10, 1671. THO. BRANKER.

[o] Possibly Lord Brereton, who gave Branker the living of Tilston in Cheshire.

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LXV.

MOUTON TO BERTIT.

à Lyon, ce 10 Juin, 1671.

J'ai reçu votre lettre du 28 Mai écrite partie de Pontoise, partie de Paris. Je suis réjoui du grand profit que vous avez fait dans la langue des Anglois jusques à vous mêler de traduire; je vous demanderois volontiers quand et comment vous l'avez apprise, et si comme Van Helmont vous savez les choses par impression divine. Vos Peres lisent les journaux que vous m'avez envoyés, où il y a de belles choses. J'ai montré au P. de St. Rig ce que vous mandez de votre voyage à Paris pour l'affaire de Moulin et de Colinet. De Moulin je vous ai ci-devant mandé que son régiment passant ici l'avoit tiré du prison, et qu'on ne sait pas si çà étoit pour le faire passer par les armes; de Colinet frere . . . . a dit qu'il croit que son affaire est faite.

Messieurs Barrow et Wallis sont de grands hommes, Gregory de même: j'admire leurs ouvrages. Je rendis votre lettre au Sieur Lacroix, qui me promit de la rendre le même jour à son addresse logée à la Pomme du pin. Je vous remercie de toutes vos nouvelles et des soins que vous prenez pour moi. La proposition que vous avez fait de la proportion de la grand apparence des objets dans la raison des foyers est fausse; j'en fis voir ici la fausseté au P. Cherubin d'Orléans, j'en ai écrit à M. Auzout, je n'en ai point de réponse. En voici la preuve. A, B (Pl. 2. fig. 1.) soient deux lentilles convexes entierement égales; H leur foyer commun; GH, HI égales; soit DH l'image d'un objet comme CF grandement éloigné de la lentille AG; IK soit égale à GF; il est constant qu'en K il se formera une image de l'objet CF, qui lui sera égale.

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Tout cela supposé, si la proposition étoit vraie, l'œil O et l'œil P verroient l'objet CF sous des angles égaux; ce qui est faux. Car dans l'œil O, qui voit l'objet à nud, les rayons d'un même point d'objet y tombent divergens, et dans l'œil P, qui voit l'objet au travers des deux lentilles, ils y tombent convergens: d'où il sera facile de prouver et de conclure que l'objet paroîtra plus grand à l'œil P qu'à l'œil O. Donc les angles visuels ne sont pas dans la même raison, que la distance du foyer de l'objectif à celle du foyer de l'oculaire.

Si vous voulez, je vous enverrai l'équivalence du systême de la Lune selon Kepler: je l'ai autrefois envoyé à Paris. Je crois même que je vous l'addressai; il n'y auroit pas apparence de le redonner aux mêmes personnes. Si votre ami d'Angleterre, notamment celui, qui observe, le désire, pour le conférence avec ses observations, j'en ferai une copie que je vous enverrai. Je voudrois avoir de bonne chose pour fournir au commerce de vos amis, mais je vous prie d'excuser mon indigence; et de croire que je suis avèc respect tout à vous.

There is no address and no signature to this letter. The allusion in the beginning to the knowledge of the English language would be very suitable to the P. Bertit, since Letter LX. is undoubtedly his own autograph. The mention of Pontoise (from whence that letter is dated) and Paris, derives a confirmation to this conjecture from Letter LXII. and what is there said of Mouton's observations, points also to him as the writer of the present letter—he was of Lyon.

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LXVI.

PARDIES TO OLDENBURG.

Monsieur, Paris, 20 Octobre, 1671.

Je ne saurois vous exprimer les sentimens de reconnoissance, que je resens moi-même pour les faveurs singulieres, qu'il vous a plu me faire, en m'envoyant les deux livres du mouvement, et en m'écrivant une lettre si obligéante. Vous pouvez vous assurer, Monsieur, que je ne souhaiterois rien plus, que de pouvoir vous faire connoître combien je vous honore, et combien je me sens votre obligé de l'honneur que vous m'avez fait, particulierement en voulant bien me permettre, que je vous écrive de tems en tems, et en m'invitant même à le faire. J'ai lu le petit livre De motu abstracto et concreto: vous me dispenserez, s'il vous plait, de vous en dire mon sentiment. Je ne ferois pas la même difficulté à l'égard du livre de M. Wallis ou de M. Wren, si je les avois lus, car je suis déja fort prévenu de l'excellence de leur esprit et de leur profonde érudition. J'ai autrefois étudié avec grand soin les ouvrages de M. Wallis De arith. infin., De sect. conicis, De cycloide, et ç'a été avec une satisfaction extraordinaire et un tres grand profit de ma part. J'ai vu aussi quelques inventions de M. Wren que feu M. Fermat m'avoit fait connoître: il n'y a rien de plus beau. J'ai été aussi charmé du tour hyperbolique, qu'il taille avec une ligne droite inclinée d'une certaine maniere, au moins m'a-t-on dit que c'est M. Wren qui en est auteur. Tout ce que j'ai vu, de la plupart de vos autres Messieurs, me paroît extrêmement beau, et me donne une grande inclination pour votre nation, et même pour apprendre votre langue, qui sera dorénavant nécessaire à tous les mathématiciens et physiciens. Comme

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nous n'avons en notre bibliothèque presque aucun de vos livres, j'ose bien m'adresser à vous pour les faire venir. M. Justel sera mon garant, et il pourra vous repondre que le prix de tous ces livres sera mis entre ses mains, comme vous le marquerez. Je vous supplie donc de vouloir nous envoyer tous les livres, qui sont imprimés en Angleterre depuis six ou sept ans, qui concernent les mathématiques ou la physique, soit en Latin, soit en Anglois. Nous avons ici Astronomia Britannica, Algebra Pell, Lower de Corde, Tabulæ Ulugh, Oughtred's Clavis, Barrow's Euclid, Wallisii Arithm. infin. &c., Commerc. Epist. de Cycloide, et les œuvres de M. Hobbes. Ainsi il ne faudra pas nous envoyer ceux-là; mais bien les autres, s'il vous plait, savoir les ouvrages de M. Hooke, de M. Barrow, de M. Boyle, de M. Morus, de Mercator, Gregory, &c. et surtout vos excellentes Philosophical Transactions ramassées depuis le commencement. Et vous nous obligerez de nous les envoyer par apres, à mesure qu'elles se feront. Il n'est pas besoin que ces livres soient réliés, on les fera rélier ici à notre mode. Je vous envoye un petit livre du P. Rapin, avec des theses de motu locali. Je voudrois de tout mon cœur avoir quelqu'autre chose à vous envoyer. Un de nos peres, qui est à Nègrepont, m'a promis une relation exacte de l'Euripe. Il m'en avoit déja écrit quelque chose, mais il me dit qu'il a fait quelques observations plus exactes que les premieres. Je ne manquerai pas de vous l'envoyer incontinent. Je voudrois bien savoir si vos Messieurs ont fait des expériences pour le son dans le vuide, tant avec les cordes, qu'avec les flutes. Dans les expériences de Florence je trouve qu'ils ont expérimenté que les cordes et les flutes sonnent dans le vuide en même ton que dans l'air, ce qui me paroît bien remarquable. Vous m'obligerez infiniment de me com-

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muniquer quelque chose de ce que vous savez là-dessus. Je suis résolu de travailler à la continuation du traité du mouvement. Le premier discours que j'ai fait n'est qu'un commencement d'un traité complet, que j'ai médité depuis long tems, et où je pense avoir trouvé quelque chose de nouveau. J'aurois bien de la satisfaction, si je savois que mon dessein peut agréer à votre fleurissante académie et à vous, de qui je suis avec respect,

Monsieur,

Votre tres humble et tres

obéissant serviteur,

PARDIES.

Je ne sais si vos libraires voudroient se charger d'imprimer deux petits traités en Latin: l'un in 12°. De antiquis mensuris, monetis et ponderibus, quæ olim fuerunt in usu apud Romanos, Græcos, Hebræos, et alias gentes clariores, cum eorum omnium reductione ad mensuras, monetas, et pondera nostri temporis: l'autre in 8°. Speculum geographicum, in quo nativa effigies terrarum ac marium conspicitur. Je connois l'auteur, qui est assurément un excellent homme de notre compagnie, et qui a de fort beaux ouvrages de physique et de mathématique tous prêts à imprimer; mais il voudroit commencer par ceux-ci, pour lesquels il a plus d'affection. Nos libraires d'ici ne veulent quasi plus imprimer que des livres François, mais surtout ils ne peuvent se resoudre à se charger de ces livres de doctrine. Si quelqu'un de vos imprimeurs me vouloit promettre d'imprimer ces deux ouvrages, je les lui enverrois, espérant qu'il m'en enverroit, de la grace au moins, une cinquantaine en blanc de chacun. Néanmoins ce ne seroit que de sa grace.

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On the outside of this letter Oldenburg has written a short note of its contents; he read it to the Royal Society at their meeting, on the 14th of Dec. 1671, and a copy of it is entered in their letter book.

LXVII.

HUGENS TO OLDENBURG.

Monsieur, à Paris, 7 Nov. 1671.

Je vous remercie tres humblement de ce que, non obstant ma paresse à écrire presque inexcusable, vous ne laissez par d'avoir la bonté de me faire part des productions de vos illustres. Il y a à la verité quelque chose qui m'a fait retarder, d'une semaine à l'autre, de vous faire celle-ci, qui est l'imprimé dont vous la voyez accompagnée. Car ce qu'il y a là-dedans des observations de Saturne, je l'avois donné il y a deux mois devant que m'en aller à la campagne, mais M. Cassini s'étant proposé de publier en même tems la suite de ses observations des tâches du soleil, la gravure des figures et autres circonstances y ont apporté cette longueur, que tout cela ne paroît que maintenant: et à mon grand regret, parcequ'ayant prédit le retour de la forme ronde de Saturne vers la fin de l'année, peu s'en faut que la prédiction ne soit accomplie devant qu'on en ait été averti,——je dis pour les pays étrangers, car nos Messieurs savent bien, qu'aussitôt que M. Cassini m'est appris que les bras de Saturne étoient revenus, je dis qu'assurément ils disparoîtroient devant la fin de l'année. Je les observai encore hier au soir, mais si foibles et obscurs qu'on avoit de la peine à les discerner; de sorte que dans peu de jours ils ne paroîtront plus de tout. Ceci confirme tout-à-fait mon hypothese de l'anneau, qui présentement dis-

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paroît à nos yeux, à mesure que les rayons du soleil en éclaircit obliquement la surface plate tournée vers notre vue. Et les apparences de cette année donneront moyen de prédire le retour de la figure ronde, avec bien plus de justesse qu'auparavant.

Je vous suis bien obligé de la construction de M. Sluse sur le problême d'Alhazen. Elle vient, comme il a bien remarqué, de la même analyse que la mienne, et n'en est pas beaucoup différente. Il me semble toujours que la mienne est la plus naturelle, à cause de la disposition des asymptotes de l'hyperbole, et il n'y a pas plus de façon aussi qu'à celle, qu'il a donnée. Mais il faut que j'en communique avec lui-même, qui est le plus savant et le plus sincere de tous les géometres que je connoisse, quand ce ne seroit que pour le prier de me faire part d'une analyse encore plus facile, qu'il dit avoir trouvée de ce même problême.

Je suis marri qu'on a tant de peine à avoir ici les livres, qui s'impriment par delà. J'ai prié le bon Monsieur V. Ruham de m'en procurer quelques uns, et nommément cette seconde partie du Traité de Mr. Boyle, mais jusqu'ici je n'ai encore pu l'obtenir. Pour la derniere partie de l'ouvrage de Mr. Wallis, j'espere qu'il aura la bonté de se souvenir de moi, quand il sera achevé d'imprimer, et il peut s'assurer que je suis un des plus grands admirateurs de ses profondes speculations.

J'attens le volume entier de vos Transactions que j'ai prié mon pere de m'apporter d'Angleterre. Je suis marri que notre Monsieur Gallois[o] ne continue pas ses nouvelles avec la même diligence que vous. Il y a deux mois qu'il est à la campagne, et que nous ne l'avous point vu.

[o] Editor of the Journal des Savans.

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Mon pere m'a envoyé une feuille de vos Transactions, c'est pag. 631[p], où vous dites des choses merveilleuses de certains vitres non sphériques. J'ai bien de la peine à croire qu'ils puissent faire un bon effet en qualité d'oculaire, et beaucoup moins en celle d'objectifs. Je vous prie de me dire quelle suite a eu cette nouvelle fabrique, dont je ne laisse pas d'admirer l'industrie; de ce qu'au moins ces vitres sont assez régulierement taillées pour faire quelque chose de plus que les sphériques, lors qu'on s'en sert à lire à travers; car c'est de quoi mon pere rend témoignage, et on attend un de cette façon.

Je me recommande à vos bonnes graces, et suis tres véritablement,

Monsieur,

Votre tres humble et tres ob. serviteur,

HUGENS DE ZULICHEM.

Nos observateurs pour l'Amérique partent dans peu de jours. Et je leur donne une pendule ajustée d'une maniere nouvelle pour observer les longitudes. Elle résistera mieux à l'agitation du vaisseau que les précédentes.

This is the original letter from which an extract is printed in the Phil. Trans. vol. VI. p. 3026. It was read to the Royal Society Nov. 9, 1671; and it appears from their Journals to have been written to Oldenburg. The cover with the address is lost.

p Vol. III. On grinding optic and burning glasses of non-spherical figures by Francis Smethwick, Esq. F. R. S.

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LXVIII.

COLLINS TO VERNON.

Dec. 14, 1671.

Your communications I imparted to Mr. Bernard, and when I had wrote hitherto I received his answer, which I shall give you. As to Harriot's papers, he saith nothing; as to those of Rawlinson, he saith that Mr. Carr[q], one of the proctors, hath those that did not go along with his books; but in short there is nothing in them desirable, being but excerpta from Euclid, Herigone, and Oughtred's Clavis. He saith, that Mr. Gale of Cambridge writes him word that Mr. Newton (Barrow's successor) hath abbreviated a sixteen feet tube to the length of a span, which is a most happy invention.

I further add, that the eyeglass is placed towards the object, the object glass from it; the eye looks in through the middle of the side, and sees all by reflection, as 'tis said, in the same perfection as, and certainly takes in much more than, when the glasses are placed in their long tube.

Sir Samuel Morland's Trumpet is now publish[ed]: oh that you had one of them, wherewith to display its own fame and the due praise of this telescope! However, I shall endeavour to send you the book by the first oppor[tunity.]

As to the book of Pere Poterius, de Ponderibus ac Mensuris compared with the standards, Mr. Bernard writes thus: "It is a necessary treatise, and if it were accurately done, I doubt not of the printing of it here at Oxford; for I design to print Mr. Greaves's foot and denarius, rendered into Latin with some additions,

[q] Alan Carr, of All Soul's College, junior proctor of the university in 1671.

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and join with you in desiring to know of Mr. Vernon his opinion, (which is sufficient,) of the worth of the book.

I add that I moved the printing of Poterius to Horne, a stationer, at the Royal Exchange, who is the principal person that prints mercantile affairs, and the chief of them that are concerned in the late impression of Roberts his Map of Commerce, which treats of the trade, weights, measures, coins and exchanges of all the most principal places in the world. He said if the treatise be good, and were here, he would be at the charge of translating and printing it in English, but would not give above 25 printed copies for the manuscript. Quære how much it may make printed.

As to Lalovera's works, Mr. Bernard writes thus: Father Lalovera was an excellent person; and if father Pardies would but send some small treatise of the said Lalovera's to you, that business would be soon at an end. I strangely came to know that Lalovera had a book in print, intituled Geometria veterum promota in septem de Cycloide libris, una cum Appendicibus. After much sending, and long expectation, one was procured from France for Dr. Wallis, who, finding the author and himself wonderfully to agree, wrote a letter to Lalovera, and sent it to me to transmit; but it being rumoured he was dead, I never sent it, having it still by me. The Doctor hath a great esteem for Lalovera; and if you please I shall send you a copy of that letter. I have wrote and sent often to P. Bertit and others to procure that book of Lalovera's, (which one Mr. Hoot assures me is very common in Paris,) and his Appendices polemicæ contra Magnanum, but could never prevail.

Now as to the printing of Lalovera's Remains. I think it were best they should come out in small

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treatises, one after another; and when the Royal Society begin to have a place for the late Arundelian library, bestowed on the said Society by the bounty of the lord Howard, with what else they have, the project, I hope, will be so laid that I may buy fifty or more of any book, they would encourage to be printed, for ready money at a shop rate, and take fifty books more to barter for others, that may be desired hence; and when these hundred books are here, to sell as many of them as I can to private persons, and barter away the rest for other books for the library, and if a loss ensue, not to fall on myself. And if the Royal Academy have the like agency, we might be mutually helpful. I hope to say more hereafter concerning this.

A bookseller here will print any book, if he [be] but sure to sell eighty or an hundred books for ready money.

I wrote a discourse to P. Bertit, to shew the ill consequences that ensue the not encouraging good books to be printed, giving him instances of many good treatises, that are by this means either quite lost, or never come to be public.

I received Billy's Diophantus Redivivus, that you sent by Mr. Nott, and immediately left it at Mr. Kersey's house for him to peruse.

When Dr. Wallis was last here, a physician alleged he did not doubt to cure him of his ague, and that he should have but one fit more, which should be but a faint one, and come twelve hours after its wonted time. The event proved true, and the Doctor hath been freed from that distemper divers months, and is in good health. He hath much enlarged his Commercium Epistolicum, which handles such unlimited problems: and if you be minded to present the Doctor or Mr. Bernard with a book, I believe one of Billy's said books

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would be very acceptable. I have the catalogue of the last term to send you. I am glad to hear of three books mentioned in it. Those are James Calvert, a learned minister of York, his Colluctationes Theologicæ cum tribus ingentibus dubiis, de reditu decem tribuum, de conversione Judæorum et mensuris sacris Ezekielis. He affirms, the ten tribes returned with the two, and that there shall be no future national conversion of the Jews. The book I would willingly present you with, might it find admittance in France. Another is, Sir Robert Cotton's Posthuma, reprinted, about state affairs. The last, Dr. John Newton's Arith.[r], wherein all the possible answers, (which he had from Mr. Dary,) in Alligation, are found. Tartalea gloried that in a certain question he had found two answers in whole numbers; afterwards Bachet, in his Problemes Plaisantes[s], found about seven thousand answers to the same question in whole numbers.

This conveys you Flamsteed's calculations of the lunar appulses to the fixed stars, (which I thought unfit to delay,) and an account of the late excellent Dutch book of naval architecture.

Mr. Hodges

The draught of this letter ends in this manner. The abrupt termination is not from any difficulty in reading Collins's writing, which is very clear.

LXIX.

HOOKE FOR HEVELIUS[t].

Sir,

I thank you very much for the opportunity you

[r] The Art of Natural Arithmetic in whole numbers and fractions, vulgar and decimal. 8vo. London, 1672.

[s] Problemes Plaisantes et Delectables qui se font par les Nombres. 8vo. Lyon, 1613 and 1624.

[t] Possibly addressed to Oldenburg, and to be communicated by him to Hevelius.

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have given me of being in any ways serviceable to a person, who has so highly obliged the learned world, as the excellent Hevelius has, both by the curious and learned pieces he has already published, and by those other great undertakings, which he has given them cause to hope for and expect from his indefatigable endeavours. And in truth, sir, upon the consideration of the subject he has designed, I being ignorant of what instruments he makes use, and of what help that northern climate affords for that purpose, I have often wished that he were furnished with as good optic glasses, as are now in use in other parts of the world, and with some good method of making use of them for determining the diameters and parallaxes of the planets, and for finding the positions and distances of the smaller fixed stars, &c. For then I could not doubt to receive, from his judgment and diligence in the use of them, better effects than what have been hitherto produced by others. And in particular I have wished that those sextants, at least, he makes use of for measuring the distances of stars, were furnished with telescopical sights, which is no small advantage for regulating and assisting the sight, which if he desires it, I shall be most ready to gratify him with any information, that the small experience I have in those things will furnish me with.

The largest glass I have several times made use of, is a spherical lens, convex on both sides, of a sphere whose radius is 60 feet, and the focus or length of the glass is near about the same length; 'tis made of a piece of glass of between $\frac{1}{4}$ and $\frac{1}{2}$ inch thick, and between 5 or 6 inches over; it bears an aperture of about 3 inches, sometimes 4 or more, according to the uses I design it for. It discovers many things not visible through a very good 36 [feet] glass; such as

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the shadow of the satellites, and the verticity of Jupiter and Mars on their axes. Some make use of two convex eye glasses, but I for the most part make use but of one, and that a very deep convex lens: convex on both sides of a sphere of inches radius. Sometimes I use such as are more shallow, but seldom any deeper. The method, by which that I have was made, was by the ordinary way in a very deep dish, and wrought on the tool by the hand without any kind of engine, till such time as it be exceedingly bright and well polished, and have received the perfect figure of the scutella, in which it was wrought, (otherwise the glass will be worth nothing,) in the doing of which there is great difficulty.

The tube I make use of is about 66 or 68 feet in length, and consists only of two long square boxes or tubes made of very thin and light slit deal, here and there bound together with very thin plates of iron, under which, within the tube, there are placed several square portions or cells of the form A, (Plate 3. fig. 2.) which serve to keep off the adventitious rays, and to keep the sides of the tube square and steady. Each of these boxes is about 10 inches square, and about 33 feet long: these two are thrust into a small square box B, in the middle about 2 or 3 feet long, made of thicker boards bound about with iron, and having two long boards CC fixed to them of about 6 feet high, and joined at the top by a piece D interjacent between them, over the top of which there is afterwards stretched a rope that serves to set the tube straight, and to keep it from warping, the manner of which will be easily understood by the figure adjoining, F a little cylinder, on which the end of the long rope FE is rolled, thereby to shorten or lengthen it, according as the bending of the tube in the middle requireth; GG another smaller

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cord wound about the former rope and the tube, by the stretching or slackening of which the two parts of the tube are reduced to a straightness as occasion requires.

This being done, the whole machine is hung by a handle, after the manner of a pair of scales, the two ends of the tube near equally counterpoising each other, and by that handle it is drawn by a tackle up to any height desirable by the strength of one man only: the whole tube not weighing at most above 200 pounds weight, and when up 'tis manageable with the greatest facility imaginable.

I have inquired the lowest rate any such object-glass will be sold for, and find it will not be afforded for less than twenty-five pounds sterling, and the eyeglasses will cost forty or fifty shillings more. If Mr. Hevelius desire any, upon his signifying his mind to me, I shall endeavour to get him the best that can be made here, and at the lowest rate.

The passage, from "F a little cylinder," to "occasion requires," is not, like the rest, in Hooke's, but in Oldenburg's hand-writing. The letters in it are not found on the original figure, but have been now inserted to answer to the description of it.

LXX.

FERMAT TO OLDENBURG.

Clarissimo viro D. Oldenburg S. Fermat S.T.D.

Quam infesta fuerit hoc anno populatio murium agris, qui in Conseranensi [u] diocesi montibus subja-

[u] It is clearly Conseranensi, but there are several instances of inattention in the writing; and the district here alluded to may be the Couserans, which are situated on the north side of the central Pyrenées.

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cent, vix enarrari potest; tanta hujusmodi animalculorum solito minorum, subrufo colore, copia subito e vicinis rupibus se effudit, ut torrentis instar segetes sternens, ipsa grandine damnosior, spicas radicitus corroserit; non te latet quid Aristotelis lib. 6°. Hist. Animal cap. 37°. et Plinius lib. 10°. cap. 65°. scripserint de soricibus agrestibus et detrimento ab iis, non solum messibus, sed etiam quibusdam populis illato; parum autem fide dignum plerisque videbitur, quod de fœcunditate murium ambo tradunt, præsertim ubi asserunt, apud Persas prægnantes et in utero parentis repertas fuisse; aut, ut Julii Scaligeri verbis utar, matres fieri antequam nascantur; utinam, inquit ille, tales apud nos vitulæ forent. Hujus vero de qua agitur rei, cujus ipse quidem testis non fui, sed ab aliis omni exceptione majoribus didici, causam inquirere, si vacat, te sagacissimum naturæ indagatorem nec pigebit forsitan nec dedecebit. Abietibus id tribuunt regionis incolæ, similem vero luem aiunt frumentis nocuisse viginti retro ab hinc annis; sed cum ibi adeo annosæ sint hæ arbores, idque tam raro eveniat, credibile est quædam præter consuetudinem tempestatis accessisse quoties hic prodiit effectus; cum igitur innumeri illi, terrigenæ, ut ita loquar, vastatores nuper orti sunt, plura, et ni fallor, insueta concurrere oportuit. Immodicum puta solis fervorem et diuturnam siccitatem vaporesque multo sale permixto e solo murium generationi aliunde apto, abiegni forte ligni putredine, præter modum attractos, qui deinde resoluti, eodemque loco recepti fuerint, sic æstate nonnunquam improviso imbre velut ebulliente pulvere stupendam ranunculorum multitudinem erumpere videmus, quæ phænomena si eodem tempore contigissent, veteris illius et fabulosæ batrachomyomachiæ nova et vera imago potuisset exhiberi. Si vero cum hæc leges, tibi in mentem veniat

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tritum illud, "parturient montes," addere merito poteris, nascetur terribilis mus, non ridiculus ut vulgo dicitur. Vale et me ama.

Tolosæ, Kal. Jan. 1672.

LXXI.

TOWNELEY TO COLLINS.

Sir,

I am very much obliged unto you for yours of Dec. 26, containing so great a variety of mathematical news, and giving us hopes of shortly seeing so many admirable things. I hope it will not be long before we receive Dr. Pell's book, now in the press, but more particularly [we] long to see Kinckhuysen's Introduction to Algebra, with those wonderful additions of Mr. Newton. In the mean time, to keep us doing, I must desire your assistance in procuring me these books. De Mensura perimetri Terræ [u], which Mr. Oldenburg told me was out before I came out of town, though as yet he hath not given us any account of it in his Transactions; for I very much long to see it, upon several scores; for I have had several thoughts how such a thing might be most exactly compassed. I have the two first tomes of Des Cartes his letters, in French, but want the third; and this I must desire from you. I had as lief have it in French as English. Another is Ludovici de la Forge Salmuriensis M. D. notæ in hominem Cartesii. The last is Ephemerides for some following years; if Wing hath any out, I should be content with them. Our carrier will be in town about Wednesday next, so that if these books could be got to Mr. Law. Halsted, by that time, I could conveniently receive them. I have not yet fully perused

[u] Probably Mesure de la Terre, par J. Picard, 1671.

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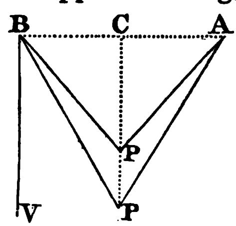
the books you sent, but in Mouton's Observations I find several things worth knowing. I have made some observations of the sun's and moon's diameters, but by another way; but as far as I can guess, his differs not very much from mine; mine I think still bigger. Mr. Flamsteed makes use of my way. I have looked a little into Synopsis Geometriæ[x], in which the author, according to his way, hath if at all, but I think not much new, nor do I think I shall find him so good in his new demonstrations as the author he slights. The other of his books I have not had time yet to look into; but by the account I received from one that hath perused it, I fear I shall not find much.

I was desired by a friend to learn from you whether L'Art de penser, ou la nouvelle langue, be translated into Latin, or that you know of any body that is about it. We have De la Forge de mente humana: this I mention for fear you should mistake me in what I writ above. I hope Mr. Oldenburg will give us a fuller description of Mr. Newton's new perspective, and inform us what kind of glass that is you look through. I suppose it a concave, and a very deep one, since it not only magnifies so much, but also by reason of the smallness of its aperture.

The problem I mentioned was this. Suppose a string, fastened at A, and passing over another point or nail at B, and having hung to it the weight P, to find out the forces V, that will sustain them at the several lengths CP.



Dr. Wallis passeth it over slightly, p. 632, in the scholium, and refers you to his second chapter.

If you could inform me which way my Lord

[x] Possibly Honorati Fabri Synopsis Geometrica. Lugd. 1669.

Brounker esteems the best way of communicating the motion to a pendulum, you would very much oblige me: you told me he had writ something of that subject, but I could not get it of Mr. Moore, so must desire your assistance. I am,

Your obliged servant,

Towneley, RICH. TOWNELEY.
Jan. 4, 1671–2.

Richard Towneley, of Towneley, in Lancashire, was a gentleman of old family, (see Whitaker's Hist. of Whalley, p.325.) His house was the resort of men of science, not only in his time, but in that of his successor.

LXXII.

VERNON TO COLLINS.

Sir, Paris, March 11, 1672.

I sent you away on Thursday last, by Dr. Eglonby[y], such of those books, you writ for, as I could at that time procure: they cost,

	L.	R.
The third volume of Descartes his letters	6	0
The two Mecaniques, at 45R.	4	10
Pascal's Triangle Arithmetique	1	5
Le Pere Labbe's Bibliotheca Bibliothecarum	1	10
In all	13	5

I did not send you Rohault's Philosophie, because it hath been lately reprinted in Holland, so it may be, by this time you have supplies of it in England. Lalovera's book nor Tannerius I cannot yet meet with. That Pascal Conics, which Des Cartes you say mentions as extant, is not so: I have spoken with a bookseller in Paris, M. de Priz, who hath printed all Pascal's other pieces; he tells me, that he hath had the

y Possibly Dr. W. Aglionby, who became F. R. S. in 1667.

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manuscript in his hand, and was once undertaking to print it, when M. Pascal died; but that effectually it never was printed: and he saith the original is now in Auvergne with his brothers, who, he saith, have thoughts this summer of coming and settling themselves here at Paris, and that then possibly he may get the copy and print it. As to Fermat's manus[cripts] I hope you have received my letter I wrote you the last post, and that will give you satisfaction. I had a great deal of difficulty to get you that third volume of Des Cartes, apart from the rest and the Mecaniques: they bind up here with the Specimina and Dioptrics, and it is hard persuading them to sell it without it. The other things you write for, as I can light on them, I will send you. Now I do not meet with any thing else. I am,

Dear Sir,

Your very faithful servant,

FRANCIS VERNON.

LXXIII.

BERNARD TO COLLINS.

Worthy Sir,

I thank you for the Italian book you intend me. I have received lately Billy's Diophantus Redivivus, but think not very well concerning it: and I shall this week shew it the good Dr. Wallis. Poterius's work I desire much to see, and more to print, if it be accurate. Your advice to embellish captain Venn's book is very good, if those that oversee the work take care that nothing be said too often. Honoratus Fabri is now close prisoner at Rome by order of the Inquisi-

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tion; so that we may justly despair of his Archimedes. Maurolycus's copy, unless joined with the learned pains of Commandinus, and corrected by Greek MSS., will not supersede an edition of that most excellent author here. Dr. Barrow's work must by no means tarry from the press, nor yet be put into the hands of untoward booksellers. Millet's and Gottigniez' Euclid I desire much to see, and not more than to see and return. I have been lately diverted from my wonted studies, and am not well recovered to them as yet; and therefore desire you to excuse me that I forbear every thing at present, but my duty of subscribing

Your very obliged servant,

March 14, 1672. E. BERNARD.

Pray advise me, if any have observed this equinox at London.

I intend here to publish Dr. Greaves's two books, viz. The Roman Foot and the Pyramids, in Latin, with some additions both by the author and my meanness. Poterius will well suit to the argument.

This letter is printed in the Gen. Dict. vol. III. p. 248.

LXXIV.

TOWNELEY TO COLLINS.

Sir, Towneley, Ap. 15, 1672.

I perceive by yours of the 9th inst. that one of mine to you hath miscarried, if not more of them; for I formerly sent you a note to the same purpose as this I now send; but howsoever, having received the wrong

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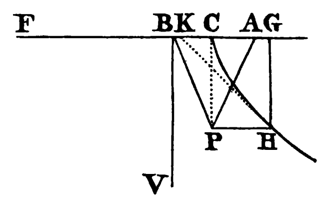
book, in my last I acquainted you that I had returned it up to Mr. Halsted, at the Rolls, and desired that you would call for it. For, as I mentioned before, we have that book, but desire this other, as also that I mentioned in my last. I have not yet seen the Transactions of March, but shall long more to see those you mention in yours of April, since they contain such curiosities both of Mr. Newton and Dr. Wallis, to whom I perceive the problem, I formerly mention[ed], was wrong-proposed, probably by some neglect of my own, for I never supposed AP to continue of the same length, but AP still to be equal to BP, as of necessity it will be when the weight is not fastened to the point P, and so BC will also be equal to AC, in which case the weight P will not rise in the circular line PO of his figure, but in a straight line PC. In which case, if you describe an hyperbola CH, whose vertex shall be C, and its transverse axis FC equal to twice AB, and right one CA, and then from any



P you draw PH parallel to CA, cutting the hyperbola in H, and then you draw the tangent HK cutting CF in K and HG perpendicular to BA, cutting it as, if need be, produced in G. I say that if V and P stand in equilibrium, there will be this analogy; V : P : : HG : GB; of which you may command the demonstration, if you think it worth your while, or it occurs not of itself to you.

I am very sorry Mr. Newton's works are so long delayed, but I hope you will use your endeavours not to keep the world any longer without them, as I shall move to advance Mr. Kersey's upon your communications.

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When your occasions will permit, I pray now and then afford a word or two to,

Sir,

Your very obliged servant,

RICH. TOWNELEY.

LXXV.

TOWNELEY TO COLLINS.

Sir, Towneley, May 13, 1672.

I am now your debtor for two letters, and hope you will accept of this in part of payment. I have received two of the books you sent me, and expect this day the other. If you please to let me know what I am behind with you for them, I shall order payment. I have shewn Mr. Kersey's Synopsis to a friend or two, and shall endeavour to procure contributors; howsoever I shall be one, and wish the other part, you mention in your letter, may soon follow: for the want of such books I think is the cause we have so few that understand any thing of algebra; I may say so confifidently of these parts. The book you mention, viz. Les Travaux de Mars, I suppose is a book of price; howsoever, I pray let me know so much of it as its rate. I am more particularly obliged unto you for your last, though I must at the same time avow my little knowledge in the nature of such series as it contains, but hope, as soon as Mr. Newton's book comes out, it will give me full satisfaction in that point, and desire to know what others have writ of that subject, and which you esteem; for I shall be in a longing condition till I can satisfy myself further in this matter. I never yet saw Lord Brounker's Exercise about equating the time of pendulums, so that I doubt not

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but Mr. Moore may find it amongst his papers; for he never sent it me, though I writ formerly to him to beg it of him. If you see him, you may be pleased to signify so much to him, with my service; for I know he is so full of business now, that I am, therefore, unwilling to trouble him; else I should not only desire this of him, but also an account [of] what tools and what ways are employed for the grinding and polishing of Mr. Newton's concave metals: I mean whether brass, copper, or iron tools; or that the artist useth the same method as for polishing of glass; and lastly, how his steel ones succeed. It is now a long time since I had the honour of hearing from M. de Sluse, but am in hopes I may again shortly receive the favour, he formerly did me, of his letters. The method I gathered out of his book is so imperfect, that I would willingly endeavour to make it more perfect before I trouble any friends with it; for I only took some notes, able upon any occasion to direct me to the construction of any proposed equation, which I find very ready. 'Tis by it that any of the following equations are fully understood, which I hope you will pardon me if I so confusedly here set down; since, after so much pains you are pleased to take for me, I cannot but in some measure perform your commands, though by the shewing my own imperfections. The grounds of these considerations came thus. After M. de Sluse had proposed to me the solution of Dr. Wren's problem more generally, that was set down in the Transactions, Num. [48. p. 961. and 53. p. 1059.] he writ, that the hyperbolical cylindroid might so be cut as to give all the sections both of cone and cylinder, and withal acquainted me with the property of an hyper. he had used to find them, and proposed to me the finding them, upon which I thus proceeded.

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Let QBSR (Plate 2. Fig. 3.) be an hyperbolical cylindroid, or rather the figure per axem. Let I be the centre of it, and DE, FG the asymptotes, IO the axis, and from the vertex O, OR perpendicular to OI, meeting the asymptote in R. Then let the solid be cut by another perpendicular plane, so that the common section be AXP. Then having made the little rectangular triangle *glm* so as the angle, comprehended by *l*, *g*, be equal to AXO, and making OI $= b$, XI $= f$, RO $= d$, [NX $= e$,] you will have NK $= \frac{le}{g} + f$, and KI $= \frac{me}{g}$. And therefore out of the nature of an hyperbola, this analogy, $dd : bb :: \frac{mmee}{gg} : \frac{bbmmee}{ggdd} =$ KL$^2 - bb$; and consequently $\frac{bbmmee}{ggdd} + bb =$ KL^2. And again, supposing KL part of the common section of the former plane, and another parallel to OI, which will, being a circle, give this equation,

$$\frac{bbmmee}{ggdd} + bb = aa + ff + \frac{2fle}{gg} + \frac{llee}{gg}, \text{ that is,}$$

$$bbmmee + ggddbb = ggddaa + ggddff + ddllee + 2ddglfe, \text{ or}$$

$$\frac{bbmmee - ddllee}{ddgg} + bb - ff - \frac{2fle}{g} = aa.$$

Thus far I proceeded, and out of this equation found that the section might be either circle, parabola, hyperbola, ellipse, [or] triangle, and, before I heard from M. de Sluse, also parallelogram. For when $bbmm = ddll$, then the equation will be $bb - ff - \frac{2fle}{g} = a$, ad parabolam; but if f were also $= 0$, the equation would be $bb = aa$, ad parallelo; but if bm > than ll, then ad hyperbolam; if less, ad ellipsim.

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And this solution of M. de Sluse his problem I took the boldness to send him; but how far short I was of his speculations you will best see by the answer he was pleased to give me, which I have here set down as near as I could render them out of his courteous letter, where, speaking of the former equation, he thus goes on: "Which doth shew the three sections, as "you have very well remarked; but if you examine "the case, in which the section is an hyperbola, you "will find something more: in this case $bbmm$ is "bigger than $ddll$; and ordering the equation, we "shall have,

$$\text{“}\ \frac{ggddaa}{bbmm - ddll} = ee + \frac{bbggdd - ffggdd - 2gddfle}{bbmm - ddll};$$

"which shews two different constructions of an hy- "perbola, as you may see by what I remarked on the "second and third propositions of my book; and "in one case the axis of the hyperbola is AX, and "in the other perpendicular to AX; and when 'tis a "straight line the section is a triangle, which happens "when $ee + \frac{bbggdd - ffggdd - 2gddfle}{bbmm - ddll}$ is equal to ee, "some known quantity, or, which is the same when $\frac{bbggdd - ffggdd}{bbmm - ddll}$ is equal to the square $\frac{2gddfle}{bbmm - ddll}$, "out of which equality, by right ordering the equa- "tion, you have $mmbb - mmff = ddll$; and you may "afterwards demonstrate that the line XA, in this "case, touches the hyperbola OQ, being prolonged if "occasion require.

"You may also, by your first equation, find all the "sections possible, when the line AX passeth through "I, the centre of the solid, in only taking from it all "the parts in which the letter f is found, and reducing

"it to this, $\frac{bbmmee - ddllee}{ggdd} + bb = aa$; as it appears "at first that if *bbmm* be equal to *ddll*, that is to "say, if AX is the same as DI, there will remain "$bb = aa$, and the section will be a parallelogram, "which is the remark of Dr. Wren; if *bbmm* is less, "it will be an ellipsis; if bigger, an hyperbola, but "which will have its axis perpendicular to the point I.

"But if you have a mind, out of the said first equa- "tion, to draw those of the sections when the line "AX is parallel to TV, you need only to transform it "into this, $\frac{bbee}{dd} + bb = aa + ff$, by taking away all the "ratios of *g* to *m* and to *l*. And this equation gives also "two constructions of an hyperbola, and two opposite "triangles; for if *b* be equal to *f*, you have $\frac{bbee}{dd} = aa$, "which shews that when AX passeth through RO, "or that it toucheth the hyperbola OQ in the vertex, "the section are two opposite triangles, and joined in "the same point O; and if *f* is less than *b*, they are "two hyperbolas reversed as those of the section per "axem VI; and if it be bigger, they are two oppo- "site hyperbolas, like those in the cone; and 'tis thus "that I found all that I signified unto you some days "since; from whence it appears that this solid hath "all the sections both of the cone and cylinder, which "made me call it cono-cylindrus."

Thus far this gentleman, by which you may see how fully he considers equations, and what consequences he draws out of one equation, so that there is no case of local problems which the solution of this comprehends not, and therefore I am certain it cannot but be grateful unto you. Howsoever I must beg your pardon for the faults which, in the haste, I am

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in fear I have committed; for I have hardly time to read it over: and let me beg the continuance of your correspondence with your obliged friend and servant,

RICH. TOWNELEY.

LXXVI.

COLLINS TO DR. BEALE.

Reverend Sir, August 20, 1672.

I have received both yours, the one dated the 12th, the other the 14th instant. I am very glad you are intended to incite Mr. Strode to go on with his conic design, concerning which I have this further to add. Upon the mention of Maurolycus in one of his letters, I went to Mr. Lampaine, a merchant, that is a good musician and lover of mathematics, who formerly lived at Messina, to know if he had yet obtained Maurolycus his Conics thence, which I formerly desired him to send for, of which Viviani, page [9. Præf. in Divinationem Geom. de maximis et minimis] makes this mention.

[Abbas Maurolycus Messanensis, duobus libris, quintum et sextum Apollonii tunc irrepértos supplere, ipsorumque argumenta divinare conatus est, (quo autem felici eventu equidem nescio) atque hi libri commentariis subjiciuntur in quatuor Apollonii priores]

He forthwith brought me the book, and gave me leave to lend it to Mr. Strode. It is not half so big as Commandinus his Apollonius, was writ by Maur[olycus] in 1547, which was eighteen years before the edition of that of Com[mandinus], and contains the first four books of Apollonius, in proper order, and two more supplied by Maurolycus. I presume Mr. Strode hath not seen it, nor have I heard of any other copy of it in England. Dr. Barrow hath epitomised, in his Euclidean method, the four first books of Apol-

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lonius, altering such demonstrations he thought fit, the which he left a year since in my hands to get printed; and now Horrox's Remains, with an accurate theory of the moon, are just coming out of the press. This is immediately to succeed, to which Mr. Bernard, of Oxford, will annex the three latter books of Apollonius, whereof (as well as the four former) there are two copies in the Oxford libraries, the one of Beni Moses, the other of Abdolmelec, with the notes of Eutocius upon one of them: either of them much better copies than that corrupt one of Ecchels and Borellius. When this is done, we shall have Dr. Barrow's Archimedes and Theodosius to succeed, with about thirty lectures of his of the nature of mathematical sciences, and the several methods of invention and demonstration; yet this Apollonius, not being properly analytical, will not hinder but rather further Mr. Strode's design, and therefore I shall the more hasten it, and send him the sheets as they are done. I have more to say of conic authors, but that must be reserved for another occasion. Hence you see there is no need of hastening their design about the ancient mathematicians at Oxford. As to Pappus, (there is a late second edition of that of Commandinus,) Dr. Pell hath made notes, as I have heard him affirm, on that author; but to incite him to publish any thing seems to be as vain an endeavour, as to think of grasping the Italian Alps, in order to their removal. He hath been a man accounted incommunicable; the Society (not to mention myself) have found him so: had they not, possibly they might have recommended him to a pension from his Majesty of France, there being an intimation from the Royal Academy to allow two or three pensions to meet persons. As to his knowledge, I take him to be a very learned man, more knowing in algebra, in some re-

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spects, (which I think I can guess at,) than any other, and they in other respects than he; but as to other parts of the mathematics, I grossly mistake if divers of them do not parasangis bene multis surpass him; his Idea[z] I have, and to his assertions therein I wish they be not as improbable presumptions; they were severely censured by Des Cartes, a man exceedingly deceived, as might be instanced, with the like conceits of himself. Mr. Haak, of the Royal Society, hath the censure, which, being epistles of Des Cartes to his friends, I wish they had passed the press amongst the rest; but Mr. Haak, being an admirer of Dr. Pell, will not impart them. All Pell's problems, in his late Algebra, will be new wrought over and much facilitated and abridged by Mr. K[ersey]'s Algebra, which will eclipse one of his assertions, or convict him of negligence. I could give many other instances: he boarded long at my house, and I wish that he and his patron[a] were out of my debt.

As to Euclid, we have many good ones, besides the late one of Claudius Milliet de Chales, which is not yet come over. As Billy writ a book, entitled Diophantus Geometra, so I wish there were a good one, entitled Euclides Analyticus. How such a one may be easily collected, I may hereafter write in another narrative to Mr. Strode. Our booksellers are desirous of a body of Perspective, Catoptrics, and Dioptrics in English, about which there may be a narrative: and I hope the ingenious Mr. Flamsteed, of Derby, will take pains therein.

As to Diophantus, the late ones of Fermat are now here to be had; Billy, since it, published a book called Diophantus Redivivus; but you will have that which

[z] An Idea of Mathematics, 12mo. London, 1650.

[a] "The L. B." [Lord Brereton] was written here, but the pen had been drawn through it.

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is better than both or either in Mr. Kersey; Rhonius, Pell's scholar, hath prepared a specious Diophantus, and treatises of Perspective, Catoptrics, and Dioptrics for the press.

As to Archimedes, they have nothing of his in Oxford library; there is one, designed by Borellius, to wit, that of Maurolycus; another by Fabri; but to what end should a comment be on that author? Were it not better to shew how exceeding intricate the methods of the ancients are, how universal and easy those of the moderns? Compare a few of many passages out of authors.

Tacquet, on the principal proposition about the measure of a sphere, and its surface, says thus;

Demonstratio jam illata hujus propositionis et sequentis, penitus diversa est ab ea qua usus est Archimedes, quæ quidem valde subtilis et ingeniosa est, sed prolixa et ardua, ad quam videlicet adhibentur duo manifesta, et undecim propositiones præter alias non paucas, a quibus illæ dependent.

Slusius, in a letter to Mr. Oldenburg;

Qui enim, in Archimedem commentarios scribere hactenus moliti sunt, non satis feliciter operam impendisse videntur. Memini me olim ex uno aut altero lemmate totum librum secundum deduxisse, nec ignoro similia præstari posse in aliis, cum admodum adolescens unica propositione ostenderim eandem sectionem fieri in conoide hyperbolico et cono illud continente per planum quomodolibet positum, dummodo per axem non transeat.

Riccii exercitatio geom. præf.;

Obtinebis ne diutius premam apud me quæcumque de geometria in genere disputata et literis consignata in certas propositiones redegi; et ex his illam præcipue a Torricellio, et a te quoque tantopere commendatam,

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quæ integram doctrinam triginta propositionum Archimedis, Lucæ Valerii, et aliorum, una complectitur; duasque præterea quibus totam pene Jo. Caroli de la Faille de centro gravitatis partium circuli et ellipseos doctrinam (justo volumine ab ipso explicatam) absolvo. See the account of this book in the Transactions.

Mr. Gregory's Geometria universalis, p. 123;

Totus Archimedis tractatus de sphæra et cylindro facile demonstratur ex hujus 3 ad modum hujus 46 et aliquot sequentium; liber de conoidibus sphæroidibus et tota Lucæ Valerii doctrina ex hujus 21, tota Guldini, Joannis de la Faille et Andreæ Tacquet doctrina ex hujus 35 et aliquot sequentium.

Hence you see there is no need of haste.

Vitruvius, with a Commentary, is newly finished, printed in French by Mons. Perrault, and an old Greek MS. of Arch[itecture] before Vit[ruvius] is a trans-[lation] by Valois and Coutcher.

As to Manilius, the ingenious Ed. Sherburne, Esq. Clerk of his Majesty's Ordnance, hath made an excellent English poem of it, with modern additions; and it is now printing. He was willing[b] to have disbursed

[b] Sherburne mentions the circumstance, alluded to in this letter, at p. 70 of the Appendix to the translation of his Manilius. He there says, "Bernardinus Baldus, disciple to Federicus Commandinus . . . . hath composed a history of the lives of all famous mathematicians, from Thales Milesius to Commandinus, with a chronology of the times wherein they flourished, and their several eulogies. Of which, if we may judge (as of Hercules by his foot) by that life he hath written of Hero, already published with his Belopoica, we cannot but conclude them worthy of that honour, which they yet want, that is, a speedy publication to the view of the learned world. And if the manuscript now in possession of some of the family, or a copy thereof, may be obtained, we may have then hopes that ere long the work will be communicated to the curious of this nation." From a subsequent letter it will be seen that the negociation for the purchase seems afterwards to have bid fair for bringing these papers to England; but it failed.

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£20 for a copy of Bernardinus Baldus his three voll. of the lives of mathematicians, who died but in 1617; the heirs are covetous, and demand 200 pistoles, to the destruction of a design like Stanley's.

Mr. Scot, a bookseller in Little Britain, the chief trader into France, observes, that England doth not vent above twenty or thirty of any new mathematical book he brings over; and therefore to think of printing the ancient math[ematician]s is, in my opinion, a design very hazardous to stationers: as will also be the reprinting of Borellius de Motionibus Naturalibus, whereof a sufficient number may be had from Messina. I should like well the collecting and digesting of modern writers that are scarce, and the printing of many of their works, which remain unprinted for want of encouragement, of which more by some other opportunity.

I correspond with Dr. Wallis, Mr. Bernard, Mr. Newton, Mr. Towneley, Mr. Gregory, Pere Bertit, Slusius, Borellius, &c. who are not strangers to the intelligence I send you; and as for others, they will know it soon enough, when proper; wherefore I must beg your excuse as to printing any part, at present, of such letters as I send you. I cannot say our booksellers will go through with what Mr. Kersey hath to print; and to propose more before that be done, were to put the world and the booksellers into an ill conceit of his great abilities. When his labours are finished, another printed proposal may be seasonable; but at present what I have said would disoblige some of my friends to see published. I am about to turn stationer myself; and as I have been, so I believe I shall continue, as eager as any man to get good books printed. The occasion thus: I have been employed near 2½ years under Mr. Slingesby, mint master, as secretary

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(and a member) of the Council of Plantations, and have received but little more than the tithe of my pay, a pension I had of £50 per annum, half my salary, a little while of the King, for the loss of my place, as an accountant in the Excise Office, by reason of altering the administration thereof since the late Lord Treasurer's death, and that hath been stopped these twelve months, as is likewise my wife's pay as laundress of the table linen to the Queen; the King's debts and occasions for the war diverting the money. Albeit I am exceedingly obliged to Bp. Ward for speaking to the Lord Clifford for his kindness to me, as I am to Sir Robert Moray in the like kind. And now the Council of Plantations is likewise to be a Council of Trade; and Mr. Slingesby, conceiving the trouble will be great, and the pay as uncertain, leaves his secretary's place, and advises me to leave that employment and to manage the Farthing Office, to deliver out all, that are coined, on Tuesdays, Thursdays, and Saturdays, in the mornings, in crown-papers ready tied up; the salary £50 per annum, and a fair dwelling house, which I think may be in or near Fenchurch-street, where, having a convenient shop, I intend, God willing, to set up a stationer's trade, (and have a promise of serving the Mint,) and afterwards hope to fall into the printing of books, especially some of the copies of the members of the Royal Society and some of my own, particularly one of the Modern advancement of Mathematical Sciences, and an Account of the best Authors of that kind, and some others which I intend, of which more hereafter. The last ages were too ignorant of mathematics, and possibly they might not at London know one of the best ways of making a carpenter's oval to any ratio of diameters. I send you here a construction I well like, which shews you that the com-

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mon data admit innumerable ovals about the same diameters. And though I leave the council it doth not follow, but that I may be employed in stating the general balance of trade and heads of the proposals in the inclosed paper, especially if such a person as the D. of Bucks, a member of the council, were earnest to have it done, and would urge an encouragement for so doing, otherwise to undertake so much trouble (though much to the advantage of trade and of the nation) cannot be expected from a private person.

Upon this change of employment you cannot but conceive my trouble will be great till well settled, and therefore I hope you and Mr. Strode will interpret my silence, especially as to the consideration of problems, to proceed rather from want of leisure than will. Next week I hope to return by your carrier the Idea you sent up, with Mr. Gregory's Optica promota, in which the problem, Mr. Strode writes about, is otherwise solved than in Bp. Ward's Astronomy.

I married the younger daughter of two only children of Mr. Wm. Austin, who, being one of his Majesty's cooks, when P. of Wales, was, by Dr. Wilkins' means, made and continued master cook of Wadham college in Oxford, during the late troubles, and is now master cook, to his Majesty, of the Lord's k[itchen]. I live at my said father-in-law's house in Petty [France], W[estminster], over against the Adam and Eve. He is now in Cheshire with his other daughter, and may return, if God please, a little after Michaelmas. Whilst in Oxford he was much esteemed for his great skill in simpling, gardening, planting trees, flowers, &c. which I mention, as understanding your good knowledge and delight therein.

Upon your mentioning of New England I have this to say. I have been informed that there hath

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been an excellent map of New England some years since sent over to his Majesty, but now it is not, upon diligent inquiry, to be found; there is a 4to book, printed in New England, entitled, New England's Memorial, by William Morton, being a history or journal of the settlement and transactions in that colony. Your judgment about the civil conversation of New and loose of Old England deserves remark; their rigour in requiring real grace in church members, and our looseness in a temporising Arminianism to obtain preferment, I take to be contrary to the doctrine of the church of England. I believe I have spent as much time to satisfy myself thoroughly in those controversies as I have done in the mathematics, and could wish all controversies stated in the method of the proposal about trade.

If the Universities should desire it, probably I might furnish them with some scarce books, as I have already done Dr. Wallis and Mr. Bernard.

I thank you for your communications, and the good mirth about Dr. Casaubon. His great book of Spirits I have seen, and that of Credulity and Incredulity I have.

For the next Letter.

To send Dr. Pell's Designs, and instances of non-extent of his methods, and of Des Cartes's vanity.

About Euclides Analyticus. A narrative about Perspective.

My own treatises.

A copy of Rhonius's last letter.

Books of modern writers unprinted. Jungius Bartholinus.

Ignorance of what extant. Davenant.

Fabri's Conics. Rhonii Epistola.

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John Beale, D. D. was one of the earliest Fellows who were elected into the Royal Society. He contributed a number of papers to the first volumes of the Ph. Transactions. He lived chiefly in Herefordshire till 1660, when he became rector of Yeovil, where he resided till his death. See Birch's Hist. of the R. S. vol. IV. p. 235.

LXXVII.

COLLINS TO SIR JOHN FREDERICK.

May it please your Worship, June 24, 1673.

I was yesterday with Mr. Treasurer Gibbons, in company with Mr. Godbid, a printer, who printed Mr. Dary's book, and with one Mr. Maney, who hath now a book of guaging in the press, and we did very much satisfy Mr. Treasurer concerning the abilities of Mr. Dary, both as to arithmetic and navigation, and as to his civil conversation and indefatigable pains. It seems forty boys[c] are to be taught arithmetic and navigation: Mr. Dary not knowing they were to be taught arithmetic, but imagining they had learned that of the writing-master, made the less mention thereof, in his method of teaching, but is not behind any of his competitors therein. 'Tis well known to very many that Mr. Dary hath furnished others with knowledge therein, who, publishing the same, have concealed his name; as for instance, Dr. John Newton hath lately published a book of Arithmetic, another of Guaging; all that is novel in both he had from Mr. Dary; the book of Mr. Maney, now in the press, the author will ascribe to Mr. Dary. Why I so much intercede for Mr. Dary, I have this to plead, besides his abilities and probable diligence.

[c] At Christ's Hospital.

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1. One Mr. Flamsteed, of Derby, a good astronomer, in order to the rectifying of sea charts, and the obtaining the true situation of places, takes indefatigable pains to calculate, by aid of the best tables, and publish, the time of the moon's appulses to fixed stars. Now it should be the work of the lovers of art diligently to watch and observe by night whether the time of such appulses do agree with the calculations; and, if not, what is the error, that by aid thereof a true theory of the moon may be obtained, and consequently the true longitude, distances, and situations of places.

This being too much neglected, Sir Jonas Moore and divers others have often proposed to make a yearly allowance to one, that shall undertake the same. This Mr. Dary may well do in a turret on the top of the hospital, and be assisted by some of the boys, which will conduce much to their advantage.

2. And there being a design to get books of engines, statics, telescopes or glasses, and algebra, published in English, Mr. Dary, under these obligations, can and will doubtless contribute his assistance.

3. There was lately a good book of building of ships, published in Holland. We desire and endeavour it should be in English, with other considerable things added thereto; and Mr. Dary having lived in Holland, may likewise be assisting therein. And——these are such things as I am loath to say how little I can presume to expect from others.

Mr. Dary, what he wants in his tongue, supplies with his pen. I have seen good poetry of his: he hath, two months since, obtained, by the assistance of the learned mathematician sir Jonas Moore, Mr. Sherburne, &c. a gunner's place in the Tower, which requires attendance on Tuesday and Thursday mornings,

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and to watch every fourth night; but that watching is but a being there ready upon call, for they are allowed a chamber and a bed. Their pay will be slow doubtless; and if that employment be inconsistent with this he seeks, he will, when settled, leave it.

LXXVIII.

AUZOUT TO OLDENBURG.

Monsieur, Rome, 24 Aout, 1673.

Vous aurez eu sujet d'être surpris, que j'aie été si long tems sans répondre à votre obligéante lettre, et vous remercier de l'honneur de votre souvenir: le premier manquement fut causé par un déménagement, où l'embarras et l'égarement de votre lettre me fit retarder quelque tems, et depuis, les mémoires que j'attendois des ouvrages de Baldus, et ensuite les ouvrages même, pour voir s'il y auroit quelque chose de conséquence, outre les vies des mathématiciens, me firent encore remettre de semaine en semaine, afin que je pusse vous faire tout d'un coup un récit entier de ce qu'il y auroit. Je ne laissai pas cependant d'écrire aussitôt à M. Justel ce que j'avois fait, et comme il vous écrit souvent, je me suis imaginé qu'il n'aura pas manqué de vous faire part, par avance, de tout ce que je lui ai mandé.

Les vies des mathématiciens sont en deux gros volumes, qui contiennent 200 vies, copiées sur les originaux de Baldus premier, qu'elles allassent, comme on dit en France, aux beurières, ou elles étoient, ne s'en étant sauvé que 60 originales qui furent rachetées par un curieux, qui s'apperçut de ce que c'étoit; mais comme le pizzicarole prenoit toujours apparemment

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les feuilles détachées, auparavant que de rompre les cahiers, il se rencontre que ce sont presque les plus longues qui sont restées, et par conséquent les meilleures, et sur les quelles il y auroit le plus à dire. Je vous envoie le catalogue des 60, n'ayant pas fait copier les noms des autres. Il y a, outre les vies, un petit épitome chronologique, où il y en a jusques à 367 de nommés, selon le tems qu'il a cru qu'ils vivoient, s'étant contenté d'en faire mention, sans en faire la vie, quand il n'en a pas su d'autres particularités que le nom, ou le pays, ou le tems à peu près qu'ils vivoient. Cet épitome est original de la main de Baldus.

J'ai vu quelques unes des vies les plus longues et j'y ai trouvé presque tout ce qui se sait de ces auteurs et de leurs opinions. Ayant ramassé tout ce qu'il a pu trouver, et comme il avoit fort lu, et qu'il a été plusieurs années à travailler à ce dessein, il aura sans doute oublié peu de choses. Il a fait la vie de tous les philosophes, qui ont eu réputation de savoir quelque chose en quelque partie de mathématiques, et il ne s'est pas contenté de parler de leurs mathématiques; mais par occasion, il a parlé de leur philosophie, et ce seroit peutêtre ce qu'on y pourroit trouver à redire. Comme pourtant ce ramas est curieux, je ne doute point qu'il ne fut agréé par tout, s'il étoit traduit en Latin, et que l'on y joignit quelques additions, s'il a oublié quelque chose, comme aussi la vie des mathématiciens, dont il n'a pas eu de connoissance dans les derniers tems, et de ceux qui ont véçu et écrit depuis sa mort. L'écriture originale est un peu difficile à un étranger, devant qu'il y soit accoutumé; les copies sont plus faciles, il y a seulement à redarguer quelques mots Grecs ou Latins, que le copiste n'a pas entendus: il y a quelques fautes, mais on les peut corriger, ou par les vies originales que l'on a, ou par les auteurs qu'il cite. Je man-

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dai à M. Justel pour combien je croyois qu'on les pourroit avoir. On avoit persuadé aux parens de l'auteur que c'étoit un trésor, et qu'ils en devroient avoir au moins cent pistoles; mais leurs ayant fait écrire qu'ils n'en trouveroient jamais rien, s'ils laissaient passer cette occasion, et qu'en faisant imprimer ces vies on feroit honneur à leur parent, à quoi ils devroient eux mêmes quasi contribuer, ils se sont adoucis, quoique le peu de commodités qu'ils ont, fait que ceux, qui s'en mêlent, sollicitent pour eux, comme pour une œuvre autant de charité que de curiosité. Je ne puis pas borner la générosité de vos Messieurs; mais à moins de 50 pistoles je croirois, par ce que j'ai entendu, qu'ils ne se tiendroient pas entièrement contens.

J'envoyai la semaine passée la liste de quantité d'autres ouvrages du même Baldus à M. Justel, afin qu'il la vit et qu'il vous en envoyât une copie. Il y a dedans quelques traités de mathématiques, et l'on m'a permis de faire tout venir ici, afin que je voie s'il y aura quelque chose, qui mérite, et depuis le tems qu'on les fait espérer, ils devroient être ici. S'il y a dans les autres traités que ceux de mathématiques quelque titre qui plait, vous me le ferez savoir et je verrai ce que ce sera.

Au reste, Monsieur, je vois que vous avez bien meilleure opinion du profit, que j'ai fait dans mon séjour ici que la chose ne mérite. J'ai fait au commencement quelques remarques; mais, depuis, ou le manquement de sujèt, ou la diminution de curiosité, ou d'autres applications, et quelques embarras m'ont empêché de poursuivre plusieurs choses, qui demandoient plus de commodités que l'on n'a pas dans un pays étranger, où l'on n'a pas eu dessein de s'établir, et où l'on peut dire que l'on ne fait que camper, ayant toujours été dans l'incertitude de mon retour. Comme on n'est point ex-

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cité ici par les conférences ni par l'émulation, il n'y a pas de quoi tant s'étonner que l'on ne travaille à rien de nouveau, et la facilité qu'il y a ici à passer, ou plutôt à perdre son tems, fait qu'on ne l'applique pas si utilement qu'on devroit; mais comme cela n'est pas trop avantageux, il est plus à propos de me taire que de m'étendre davantage sur ces excuses. Je vois aussi que la guerre diminue un peu de la curiosité en Angleterre et en France, mais comme on fait espérer la paix, et qu'il y a apparence qu'elle sera durable, si elle se fait, on pourra recommencer à travailler par-tout à l'envie l'un de l'autre.

Nous avons ici depuis quelque tems M. Borelli, qui travaille tout doucement à son traité De motu animalium, mais le grand chaud qu'il à fait ici cette année, en ce pays-ci, l'aura empêché, aussi bien que les autres, de s'appliquer beaucoup au travail, outre qu'il a fallu qu'il se soit établi ici, ayant dessein, autant qu'on en peut juger, d'y rester. Je lui ai fait vos baisemains qu'il m'a chargé de vous rendre. Nous n'avons point eu ici M. Malpighi, comme on nous l'avoit fait espérer, et je ne sais même ce qu'il fait. Je crois que vous en avez en Angleterre plus de nouvelles que nous. Les derniers livres de M. de Boyle, dont vous me parlez, ne sont point arrivés ici que je sache. Je les verrois volontiers. Je vous supplie, Monsieur, de m'entretenir toujours dans les bonnes graces de tous vos Messieurs de la Société Royale, et de les assurer de mes profonds respects. J'appris ces jours passés de M. Justel, avec déplaisir, la mort de M. le Chevalier Moray. Honorez moi de vos commandemens, et me faites la grâce de me continuer votre amitié, et de me croire véritablement, Monsieur,

Votre très humble et très obéissant serviteur,

AUZOUT.

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The address of this letter is wanting; but the following memorandum appears on it in Oldenburg's hand-writing.

Rec. le 27 Sept. 73.

Rep. le 6 Oct. 73. remerciais de Baldus, lui parlai des livres imprimés ici, de l'Algèbre de Kersey, de l'Archimede, Anal. de Baldi, de 4 livres de Boyle, Affin. de la flamme, des Effluviums, 3. du Froid, 4. de Gemmis. Encore des Traités de Grew, qui s'accorde avec Malpighi, du Traité de Willis, de Oper. med. sous la presse, et de l'espérance que Boyle donnera son traité des ..... et Figures; que je me déchargerai de ses commissions vers la société.

A work of Baldus was printed in 1707 at Urbino, (4to.) under the title of Chronica de Matematici, overo Epitome dell' istoria delle vite loro.

LXXIX.

CH. ANDERSON TO SCOTT[c].

Honoured Sir,

About a month ago I received one of yours from Paris, wherein you were pleased to desire me to disburse here 200 French crowns for certain MSS. of Bernardinus Baldus, containing the lives of all the mathematicians from Thales to Clavius, in Italian, two volumes in folio; to which request of yours I had answered much sooner, had not two or three weeks passed over, after the receipt of yours, before I could meet with M. Auzout, or know who had the MSS. to sell. Wherefore, having now met with M. Auzout,

[c] Described on the address as Mons. Robert Scott, Marchand Libraire, à Londres.

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and understood by him that the foresaid MSS. may be had for one hundred and fifty Roman crowns, which, of your money there, is forty-two pounds sterling and ten shillings. If you approve this bargain, and be pleased to disburse the said £42 and ten shillings to Mr. William Harcourt, who will do me the favour to consign you this letter, I will presently, upon his advice and yours, disburse here one hundred and fifty Roman crowns for the MSS. and take them into my own custody, that immediately upon your advice I may consign them to whom you shall think best to order for safe transmitting them thither. The owner of these MSS. tells me he hath other works of the same author in Urbino, which he will send for: when they come, if I find they may be for your purpose, I will not fail to give you advice. In the mean time accept my good-will, while I wish you all prosperity.

Honoured Sir,

Your most humble servant,

Rome, the 22d Nov. 1673. CHRISTOPHER ANDERSON.

LXXX.

COLLINS FOR TSCHIRNHAUS.

Mr. Oldenburg, Sept. 30, 1675.

Sir, be pleased to acquaint M. Tschirnhaus I have perused his letter, which was pleasant to me: and first he mentions the method of transformations M. Leibnitz is fallen into, which doubtless is one of the best accessions to the analytics that could be expected; and whereas he saith he did not see the same series in Mr. Newton's letter; that is true, but nevertheless his method is as well applicable to a sector of a circle as

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to a zone, arch, or segment. But possibly [he] neglected the same, as conceiving a series for the area of a whole circle would be extreme slow or tedious in its result. And, though probably there may be given in many cases foundations, that are both more easy and universal, yet even those of Mr. Newton may become of frequent use in arithmetical problems, as even in that of Dr. Davenant, which I mentioned to M. Tschirnhaus before his going over; namely, the sum of the squares and sum of the cubes of four continual proportionals being given to find the proportionals themselves; how this and other problems of the same bran have been performed by Mr. Baker, be pleased to impart that abstract, or a copy of it, which you had from me; the full process whereof hath been imparted by Mr. Baker in an exercise of eight sheets of paper, which shews Mr. Baker to be a learned analyst, and a person fit to labour in discovering canons for the surd roots of equations. If M. Tschirnhaus shall think fit [it would be desirable for him] to print and impart his method for the same, (which had been done by Mr. Gregory, had not death prevented,) and the which he may safely do to you his countryman, that his fame may be upon record in the register of the Royal Society: however, be pleased to thank him for those assays of his method already sent.

If Dr. Davenant's problem had been propounded either as to more proportionals, either arithmetical or geometrical, or to the sums of the same or other powers, such problem, according to common algebra, would have been most intricate and laborious: whereas such problems may be reduced to an infinite series by aid of Mr. Newton's principles. I shall mention how, in the problem under discourse.

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	1	Hyp. $aa + aarr + aar^4 + aar^6 = Q$, the sum of the squares.
	2	Hyp. $a^3 + a^3r^3 + a^3r^6 + a^3r^9 = C$, the sum of the cubes.
1st $\times \dfrac{1}{aaQ}$	3	$\dfrac{1 + rr + r^4 + r^6}{Q} = \dfrac{1}{aa}$.
2d $\times \dfrac{1}{a^3C}$	4	$\dfrac{1 + r^3 + r^6 + r^9}{C} = \dfrac{1}{aaa}$.

If the 3rd equation be cubed, and the 4th squared, they will both ascend to the 18th potestas of the unknown symbol, whence, by cross multiplication, the problem is brought to an equation of such high dimensions as are unpleasing; but if the square root of the 3rd and cube root of the 4th be extracted, according to the first theorem in Mr. Newton's letter, you will then have two series, each equal to $\frac{1}{a}$, and consequently there will be a series, whose root will give r or rr; and the root of such series, extracted in species, will leave a standing canon for attaining the root sought in a problem of this kind, and probably the coefficients will keep in such progression, that it will be easy to continue the series ad libitum.

Let my thanks be returned for his communications about knowing the habitudes that coefficients must have, that two or more terms, upon new forming, may be taken away together, which doctrine is handled in our Harriot, from whence I took a hint to make out more of those habitudes; the other about knowing when the roots are in arithmetical, (or geometrical progression, and thereupon the depressing the equation,) ariseth from the comparison of the terms of equations which doctrine is also in Harriot, but in Kinkhuysen applied to many other cases. The occasion of this discourse being grounded on Mr. Gregory's assertion,

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that, in an arbitrary equation, it is impossible to take away any two terms at pleasure without elevating the equation to higher dimensions, and M. Tschirnhaus not seeing the grounds of such assertion, doubts of the truth thereof, be pleased to suggest that I have no demonstration of the impossibility, but yet really believe it on Vieta's assertion; because I have often tried and found that such equations as have the habitudes cannot, by any hitherto known rules of art, viz. neither by increasing, diminishing, multiplying or dividing the unknown roots, be forced to lose such habitude; nor equations that want the habitudes be so far reduced as to attain them, though indeed they may be made to approach nearer and nearer thereto till they fly off again.

Mr. Gregory in his letters asserting, that by elevating any equation he could take away its intermediate powers, M. Tschirnhaus is in doubt whether this assertion or method be not the same with another following, concerning the attaining the surd roots of all equations: I answer not, and give the following narrative of the occasion of Mr. Gregory's letter.

I desired to know his opinion concerning an assertion of Dulaurens, in the Preface of his Specimina Mathematica, printed at Paris, 1667, viz. Hanc methodum sequitur alia multo admirabilior, per quam cujuslibet equationis terminos omnes intermedios auferre licet, et quidem duos aut tres per ea quæ huc usque reperta sunt, verum ad plures quam tres auferendos necesse est ut nova reperta dentur, quæ generalis hujus methodi usum latius extendant. Scio hoc paradoxum multis visum iri, qui sibi persuadent, omnia quæ humani ingenii viribus acquiri possunt, jam ab iis, qui de analysi nuper scripserunt, inventa esse, aut facile ex eorum principiis deduci posse; and withal

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acquainted him with an usual assertion of Dr. Pell, that after the limits of an equation were obtained, then offering any resolvend or homogeneum comparationis whatever, he could by direct operations give the logarithm of the root sought, not erring an unit in the last figure of the said logarithm. I offered my opinion in following letters concerning this, that if it were done, as Mr. Gregory seemed to suggest, by advancing the equation to higher dimensions, that then doubtless the coefficients of the several inferior degrees must be advanced repectively to higher dimensions, and at last be added or subtracted according to the signs, which was inconvenient work for logarithms. And that I conceived, to shun this, Dr. Pell might use a table of antilogarithms; and of such a table take this account.

Between the years 1630 and 1640, Dr. Pell and one Mr. Warner, deceased, mentioned in Mersennus, agreed to make a table of antilogarithms, which were to be called Antilogarithmi Pellio-Warneriani; and accordingly such a table was computed, and left in the hands of Dr. Thorndike, deceased, and cost Mr. Warner above 400 crowns the doing: as to the table itself, it is a table of 99998 geometrical mean proportionals, between an unit and 100000, each to eleven places of figures, elegant, in a large folio[d]; and considering the logarithms were already made, and more proper for compound interest and annuity questions for all ratios,

[d] This antilogarithmic table is particularly mentioned by Wallis, (Opera, vol. II. p. 63.) Thorndike was a prebendary of Westminster, and the papers were left by him in the hands of Dr. Busby. Birch (Hist. of the R. Soc. vol. IV. p. 447.) says, that, in June 1755, he procured for the Royal Society four large boxes from the trustees of Dr. Busby, containing papers of Pell and Warner. These are not now to be found; but a number of Pell's MSS. are in the British Museum, among Birch's collections: they do not, however, comprise the antilogarithmic table, of which Collins speaks.

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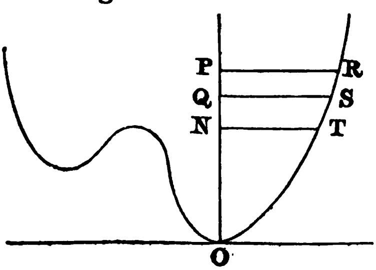
I could not conceive but that this table was made properly for algebraical uses in resolving equations; what use it was intended for Dr. Pell is not free to disclose; none of his friends here can render him communicative. We hope the doctrine of taking away intermediate terms is preserved amongst the papers of Dulaurens and Frenicle, and that M. Ts. will be instrumental in recovering or restoring the same.

For my own part, after the limits of equations are had, I can then by approaches get any of the roots, and such as they are I here think fit to impart.

If the second term of an equation be wanting, the penultimate may be removed into the room of it by the 13th chapter De Beaune, and that without fractionizing according to Kinckhuysen, in which case either limits are not necessary, or if they be, may be found by an equation two degrees lower in dimension than that proposed. In this case one of the dioristic limits is lost, the curve that is the locus of the equation touching the base line.



When dioristic limits are either not required or given, I proceed by approach thus. Out of two roots, assumed as near that sought as can easily be guessed, raise two resolvends, and conceive them set off from O (the vertex of a limit) to N and P, and their roots NT, PR, are to be raised upon them as ordinates, (for negative roots all the signs of the odd powers, only, in all equations must be changed, and the roots set off as ordinates on their resolvends the contrary way,) and, supposing OQ to be that resolvend, whose root QS is sought, say,

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As the difference of the logarithms of ON and OP
Is to the difference of the logarithms of NT and PR;
So is the difference of the logarithms of ON and OQ
To the difference of the logarithms of NT and QS.

This work makes one of Mr. Gregory's intermediate parabolas, mentioned in his Geometriæ pars universalis, to pass through the points R, T, O; O being the vertex of such parabola, which doth almost coincide with the locus of the equation. If the root thus found be not near enough, the like work must be repeated by aid of that now discovered. If you conceive a chord line to join R,T, and a touch-line to be drawn at either of those, then you have a majus and a minus for the root without use of such supposition. For raising resolvends out of a root, without raising the powers thereof, and for depressing the equation, (a root being known,) without a thorough division, I imparted some expeditious ways when M. Tschirnhaus was here.

Now I come to speak of the theorems about angular sections imparted or mentioned in M. Tschirnhaus's letter, which are (as to my knowledge) altogether new, and different from any thing, in Dr. Wallis's manuscript about that argument, the which was by him written about the year 1648, consisting of five chapters: the first of duplation and bisection; the second of triplation and trisection, and so on to quinquesection, the chief thing in it, that made me mention it in the former papers, being this: A peripheriæ quovis puncto ad singulos inscriptæ figuræ cujusvis equilateræ angulos ductæ rectæ, radices sunt equationis totidem dimensionum, quot sunt inscriptæ latera, alternatim affirmativæ et negativæ, omnesque simul additæ se perimunt.

The Doctor was never solicitous to have it printed;

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but I writ to him three years since to send it up in order to the press, rather than it should be stifled.

Albeit I have commonly asserted that for the making a table of sines (were it now to be done) the doctrine of angular sections is of little or no use; albeit I do not say this to hinder contemplations about the same, but rather to promote the main end thereof, the solving of equations thereby, and that for these reasons.

1 Here transcribe them out of my letter to Dr.
2
3 Wallis, beginning thus: 1. If divers arcs be equi-
4 distant.

As to the instrument invented by M. Tschirnhaus for dividing an angle in ratione data, we suppose he gives an angle as geometers do, ready drawn by accident or at pleasure, and then I conceive it an instrument worthy the author: whereas here (so far as I know) we have nothing but the old mechanism, viz. to measure the angle in degrees first, by aid of a sector or opening joint, and then set off the part proportional by aid of an arch or line of chords, which one of the legs may draw after it, which part proportional may be attained by a sliding scale with log^{cal} lines upon it, which may be annexed to the other leg; but here I will a little enlarge on the use of M. Tschirnhaus's invention.

We have an instrument called the serpentine line, or, as Oughtred terms it, the circles of proportion, in the use whereof, in relation to compound interest, it is often required to divide an angle in ratione data, or an angle being given to enlarge it in ratione data. Moreover, conceive the eye at the south pole, projecting the loxodromia or rumb of a ship's course on the earth, on a plane touching the sphere at the north pole,

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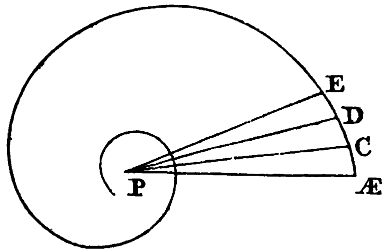
the projected curve will be a spiral line, in which, if the polar rays PE, PD, PC, PÆ, make equal angles at the pole P, those rays will be in continual geometrical proportion; and conceiving a circle



described upon P as a centre, the equal segments of the arch in the circumference, made by the polar rays, will be an arithmetical progression, suited to a geometrical one; consequently the spiral line is a logarithmic curve; and from hence the meridian line of the true sea chart may be demonstrated to be a line of logarithmic tangents, and the spiral line, with M. Tschirnhaus's angular instrument, makes the mesolabe, which our late learned Oughtred said was hitherto tenebris obvolutum.

To rectify or straighten this spiral, or part of it, as EÆ, is all one effect as to draw a touch-line to it, or to find the rumb between two places whose latitudes and difference of longitude are given; which to perform in lines is a proposition of great use, and hitherto wanting in navigation, and depends on the quadrature of the hyperbola, as Dr. Barrow, at my instance, proved in his Geometrical Lectures. Moreover such a spiral, being once well described, may serve to take away the use of compasses in Galileus or our Gunter's sector or joint for proportions, all which I thought not impertinent to hint.

Lastly, whereas M. Tschirnhaus proffers his friendship to procure what new books there may be had in Italy, be pleased to suggest, that if De la Hire's Conics printed at Paris, and Viviani's Treatise de Loco Solido, at Florence, Borellii Archimedes, or any other new

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mathematical book, be left with Capt. Arthur Maginnis, gent. of the horse to Mr. Montagu, the English ambassador at Paris, he will pay for the same, and transmit them to his and

Your thankful servitor,

J. C.

Apologize for writing to M. Tschirnhaus first in regard of his intentions for Italy, and of Mr. Newton's absence.

LXXXI.

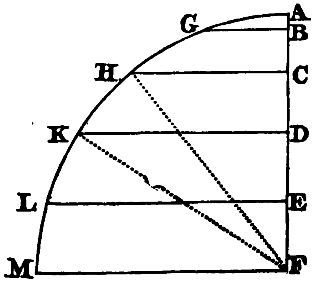
DARY TO COLLINS.

Mr. Collins,

Sir, The last time I saw you I suppose you did not fully understand me concerning the sections of a quadrant (by reason of the present tempest); but now, she being gone forth, I have enjoyed one half hour's calm, and because I would not be thought to speak in the clouds, I have done this.

Your servant,

MICH. DARY.



If the limb of a quadrant be divided into as many equal parts as you please, by right lines drawn parallel to one of the legs of the quadrant; and if you shall account the areas, intercepted between each pair of parallels, to be in arithmetical progression, you shall have the truth at both ends, and also in the middle of the quadrant; the quadrature of the circle being granted.

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Demonstration.

1. Let the portion ABG be found according to the artifice thereunto appertaining.

2. Mr. White hath sufficiently proved the parallel section HCDK to be equal to the sector HKF.

3. The area of the quadrant being granted, it is evidently the sum of all the terms, i. e. of all the sections.

4. Because the middle term HCDK is equal to the sum of all the terms divided by the number of terms, if you shall account all the terms in arithmetical progression, you shall destroy neither the first term, middle term, nor sum of the terms. Which was to be demo[d].

LXXXII.

GASCOINES TO OLDENBURG.

Sir, Dec. 15, 1675.

Wonder not to see yours of November 18, 1675, to Mr. Francis Line, answered by another hand; nor that hereafter the same happen as often as new occasions of writing present themselves. For the great epidemical catarrh, which hath ranged through so many countries, and taken away so many aged persons, hath also overcome him.

As for his second letter, of which you demand the date, and indeed a copy, we suppose it is in order to the printing of it, which his friends and scholars (of which I had the honour to be one) still hope no less nor expect, than if himself were yet alive to press it on. For, if Mr. Newton thought it not fit that Mr. Line's first letter should come forth without his own answer annexed to it, though in your words; it seems

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no less reasonable that the world should know what Mr. Line replied to that answer, than what Mr. Newton had to say to him; wherefore, that you may not be put to further trouble or search, or any demur be made on that account, I send you here another copy of the same letter, as it is written in his own hand.

You have hinted to Captain Pugh that Mr. Newton intends to shew his experiment to the Royal Society. We praise his resolution, and esteem it the only sure way to make his assertion pass for true. For being of itself extraordinary and surprising, as himself confesseth, and besides ushering in new principles into optics, quite contrary to the common and received, it will be hard to persuade it as a truth, till it be made so visible to all, as it were a shame to deny it: as indeed it will be when once attested by so renowned a body as that is.

But what Mr. Newton fancies, as if Mr. Line depended rather upon old experiments than any new trial made since the beginning of this debate, is a thing much contrary to Mr. Line's known humour and practice, and what many here are witness of. For he hath said it again and again, and called divers on purpose to see it, nor ever made difficulty to shew it to any one, who either by chance came to his chamber as he was doing it, or shewed the least desire to see the same. So, that for point of experience, Mr. Newton cannot be more confident on his side, than we are here on the other, who are fully persuaded that unless the diversity of placing the prism, or the bigness of the hole, or some other such circumstance, be [the] cause of the difference betwixt them, Mr. Newton's experiment will hardly stand. For as Mr. Line was always at home, and in his chamber, and ordinarily kept his prism just ready before the hole, so we think it pro-

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bable he hath tried his experiment thrice for Mr. Newton's once, and that in a clearer and more uncloudy sky than ordinarily England doth allow.

You say that Mr. Newton is a very modest and considering man, which we have nothing against; and your own word deserves credit: but to think him either more moderate or exact and studious than Mr. Line was, will be hard to persuade those, who were acquainted with the man.

Wherefore in this let us suppose them equal; that they were both great scholars in their kind; great lovers of truth and haters of contest for itself; that both trusted to nothing but to their eyes and experience, nor delivered any thing but what they thought they had truly found. And then it will remain only to try the experiment yet more closely and diligently, and before more witnesses, and perhaps more in the same way by means of Mr. Newton's last directions, till at length truth appear: wherein Mr. Newton may still perform his own part, but Mr. Line must now depend on the help of others, which yet I dare assure you will be such as had rather side with Mr. Newton, if he be in a truth, than second Mr. Line in an error. And, therefore, you may assure Mr. Newton, that when the sun and season shall serve for the trial, nothing shall be certified to him, but what we find by real experience in following the directions given us in his last, which if they be different from what he hath printed, that belongs to him to reconcile.

In the mean time, what hath passed betwixt them hitherto on both sides, will fully, we hope, come to light, that the world may see what these two learned men had to say to each other.

Sir, your humble servant,

JOHN GASCOINES.

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Sir, if you think fit to write any thing, your address may be,

A Monsieur Mons. Gascoines, Gentilhomme Anglois, demeurant chez Mademoiselle Hagerston.

This letter is directed to Monsieur Grubendol, à Londres; but there is written on the side of it, "Mr. Gascoines' letter to Mr. Oldenb." with a note of part of its contents; and there are the following memoranda on the back in Oldenburg's hand.

Gascoines' letter. Received Dec. 28, 75. Answ. Jan. 18, 75–76.

Written to him Mr. Newton's further directions in his letter to me of Jan. 10, 75–6: and signified to him, that if there was any fault in printing Mr. Line his letter, and Mr. Newton's reflections thereon, it was mine altogether.

May 4, 1676, signified to Mr. Gascoines the success of the controverted experiment made before the R. Society, and proving as Mr. Newton had affirmed it would.

This is the letter referred to by Newton, and from which an extract is inserted in the Phil. Trans. vol. X. p. 503. It was read to the Royal Society on the 30th of Dec. 1675.

LXXXIII.

D. GREGORY TO COLLINS.

Worthy Sir, Kinardie, 10 Junii, 1676.

Perceiving the favour and good correspondence betwixt my deceased brother Mr. [James] Gregory and you, I thought it my duty to salute you, though not acquainted, and to make proffer of my best endeavours toward your service, conform to the way of my weak

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ability, which shall not be wanting when I know how to manage it to your satisfaction. Among other things I observe among his papers a bond belonging to you of some five or ten pounds st. . . . . . . how be Sir Andrew Dick . . . . . one attorney. I would have sent it by this gentleman the bearer, (for sure occasions from this place are very rare,) but that at present it's lying at Abd. [Aberdeen?] But upon your order it may be transmitted or delivered to whom you please. I know Sir Andrew returned not to Scotland till [a] very short while before my brother's removal, else he had done some diligence in your concern. If there be any balance of accounts resting betwixt you and my brother, let me know, and what is due to you thereupon shall be readily remitted. I must also make bold to desire the favour to have a copy of my brother his resolution of Kepler's problem he sent you, with a sheet (or the like) he wrote de maximis et minimis; for albeit I find them mentioned in some letters, I find no copy hath been [kept] by himself: or if any other paper containing any thing of art from him, which I have not observed, be by you, they would do me great pleasure to see them. Thus, in expectation of your commands, I rest,

Sir,
Your very affectionate
And humble [servant],
D. Gregory.

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LXXXIII.

HALLEY TO OLDENBURG.

Sir,

I received yours, and am very glad to hear that my Lord Brounker has perused my paper, and am fully persuaded that his Lordship is satisfied as to the truth of every thing in it, though he desire a demonstration of it from me. I had at first some thoughts to have given it, but afterwards, considering that it was to be read by persons much more knowing in mathematical things than myself, I doubted not but what I there asserted, and which I will, as you desire, demonstrate, would at first sight appear evident to them; wherefore I endeavoured as much brevity as possible, and it seems thereupon fell into obscurity. I could wish you had been so particular as to have mentioned what part of my paper my Lord thought to need a demonstration, that I might more largely insist upon it; but since his Lordship might speak in general terms, I shall evince all that to me seems of any difficulty, as you will find in the conclusion of my letter, whereto I likewise add an example of the work. But I see no necessity of publishing any such demonstration; for Cassinus only gave the rule in his method for this purpose, without any proof, and Mr. Mercator is full out as obscure in his treatise of Mars; yet in this matter I shall subscribe to your better judgment. However I desire the publication of it may be as little as possible deferred; the reason, that induced me to it, requiring that it should have been out long since, and I do not question but you will do me the favour to publish it as soon as you can conveniently.

My mention of Mr. Mercator puts me in mind of a

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thing, which may be acceptable to you, that is, that in Numb. 57, (if I mistake not,) or the Transaction for March 1670, in his example of Cassini's and his own method, whereby he would shew the insufficiency of Dr. Ward's theory, he hath mistaken in his calculus, and neglected to account for the motion of the aphelion, which in years so far distant as those he uses is very considerable, as also to reduce the places of the planet from the ecliptic to his own orb; both which cause that great error in the place of the aphelion by him computed; yet the eccentricity, which those observations are unfit to determine, will be near upon as much as he saith faulty, but the error will be the other way. If you shall think fit, you may let Mr. Mercator know as much; but I desire that, if you do, you would please to conceal my name.

Mr. Flamsteed has done me the favour to send me a copy of Monsieur Cassini's letter, as likewise the most material part of that of Hevelius, but I hope to be more particularly informed by your next Transaction. I am very glad to hear that Hevelius is about publishing his catalogue of fixed stars, and his observations. I doubt not but that they will be mended by Mr. Flamsteed, whose instruments excel most of his in magnitude, and all, at least in my judgment, in contrivance; but the consent of several observers will more certainly evince the truth. As to Cassinus, I cannot learn what he is busied about, they being so very close as not to make public any thing considerable of what they do there. I have been informed that there is in the press at Paris a book of astronomical observations made in foreign parts, the most by one Rheiner [Richer?] in America, entitled Peregrinationes Astronomicæ. I desire that you would please, when you write, to inquire of your correspondents there con-

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cerning the nature of that book, and particularly if there be in it a complete catalogue of all the southern stars never visible in our horizon. For if that work be yet undone, I have some thoughts to undertake it myself, and go to St. Helena, or some other convenient place, where the south pole is considerably elevate, by the next East India fleet, and to carry with me large and accurate instruments, so as to be able to make a most accurate sphere of fixed stars, and complete our globes throughout: nor will that be all; but by comparing observations made there and here, the proportion of the moon and earth, with their distance, will be more exactly, than any other way, found. But if it be already done, I would not then meddle with it, though I would very willingly do something to serve my generation; and here I can do nothing but what will be rendered wholly inconsiderable by the greater accurateness of the three great promoters of the astronomical science in our age. I will willingly adventure myself upon this enterprise, if I find the proposition acceptable, and that the East India Company will cause me to be kindly used there, which is all I desire as to myself, and if I can have any consideration for one to assist me. This, Sir, I propose to you, desiring your advice as to what inconveniences there may be, and if you think what I propose may meet with any encouragement.

I have nothing at present worth communicating, unless it be two observations I made here since my return; the first was the moment of the emersion of the 22ª ♒ from the moon, on June 19th, at 11ʰ. 20ᵐ. 50ˢ., which Mr. Flamsteed at Greenwich[c] observed at 11ʰ. 27ᵐ. 11ˢ., by which two moments, with consideration of parallax, I deduced the difference of our meri-

c Hist. Cœl. vol. I. p. 204.

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dians $5^m. 56^s.$, but of London and Oxford $5^m. 30^s$; the distance between Oxford and Greenwich in a great circle of the earth $57\frac{1}{2}'$, equal to 66 English miles, according to the French measure of the earth; but the distance measured in Mr. Ogleby's Britannia is 57 from London, and Greenwich is 5 miles more, which together make 62 miles, but 4 less than what I deduce from lunar observations; by which you may see how well that way of finding longitudes performs, if in both places the moon be accurately observed, or if we ever shall have lunar tables that will punctually represent the moon's motion. The other was the next night, June 20, when at $11^h. 57^m. 55^s.$ was the moment of the emersion of the 6th of ♓, as near as I could estimate, having no instrument to measure, 70 degrees from the northern cusp, then something obtuse: and at $12^h. 1^m. 19^s.$ the star was in a line with Apollonia and Pentadactylus of Hevelius his scheme[d]. This observation was denied Mr. Flamsteed, thick clouds covering his horizon. One thing more I had almost forgot; that is, that on Wednesday night last the sky being very serene, and the wind still, by several meridian altitudes of fixed stars on both sides [of] the zenith, I found the height of the pole here but 51° 43′, which Dr. Bainbridge had long since stated 51° 46′,[e] but it is certainly less by two minutes.

One more request I must make to you, that is, that if it may be done conveniently, and without trouble to you, you would please to insert a line in your next letter to Dantzic, to procure me Hevelius's two large

[d] Selenographia, p. 229, 232.

[e] In another copy of the letter this last part of the sentence stands as follows: "The difference is but little, but was sufficiently sensible by my quadrant; almost all consenting within 42′ and 44′, and the mean of fourteen several observations falling upon 51° 43′, which I hardly think half a minute faulty."

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selenographic tables, at what rate soever, for they are things I cannot well be without; and the book is so chargeable, and in my opinion of so little worth, that I care not to buy it.

And now it is high time to make apology for my too long exercising your patience, and for so impudently adventuring to request kindnesses of you, when I am utterly unable to make any proportionable return of gratitude; but I was encouraged to do it by the several favours, you were pleased to shew me, when I was last in town; and I have almost the confidence to think that you may not esteem it any trouble, to add these few more to those many obligations, you have already laid upon me.

I delivered the letter you entrusted me with to Dr. Wallis, who entertained me very kindly, and I had a great deal of discourse of an astronomical nature with him; and he, at my departure, told me he would gladly see me some other time; wherefore I reckon myself much engaged to you, for giving me [the] opportunity to come to the knowledge of a man I so much esteem.

The things that in my paper may be thought obscure, and wanting a demonstration, are, as I suppose, first, the rule for finding the length of any focal ray of an ellipsis, the transverse diameter, distance of the foci and the angle made with the transverse diameter being given (or, in astronomical terms, the eccentricity, mean distance, and angle of coæquate anomaly, to find the distance of the planet from the sun). The rule I gave was this. If the angle of coæquate anomaly be acute, the difference of the mean distance and that part of the eccentricity, which bears proportion to the whole as cosine of the given angle to radius, is to the difference of the eccentricity and mean distance, as the sum

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of the mean distance and eccentricity is to the distance at that anomaly required. But if the angle be obtuse, the first term of the proportion is the sum of those parts, whereof in the former it was the difference, the second and third remaining the same.

The method by which I found these theorems was this. In the ellipsis (Pl. 2. Fig. 4.) HPGD, GH and its half CG, the mean distance, is given, call it b; so is FS, whose half CS call c; and the angle HSP acute, or HSD obtuse, is likewise known, its cosine divided by radius call s; and in the first part of the work let SP, in the other SD, be called a; and since it is evident, in all writers of conic sections, that in an ellipse the sum of any two lines from the two foci to the same point in the circumference is equal to the transverse diameter, $2b$ will be equal to SP + FP, or SD + FD; and thence, by the 36. 3 Euclid, it follows,

In the first case,

1. $2c : 2b :: 2a - 2b : \frac{2ab - 2bb}{c} = \text{SK}.$

2. $c + \frac{ab - bb}{c} = \frac{cc + ab - bb}{c} = \text{SR}$ in schemate.

3. $sa = \text{SR}$, ergo $= \frac{cc + ab - bb}{c}$, juxta Trigonom. regulas.

4. $csa = cc + ab - bb.$

5. $csa - ab = cc - bb.$

6. $ab - csa = bb - cc$, ergo.

7. $\frac{bb - cc}{b - cs} = a$: which theorem, reduced to a proportion in words, is that which I gave to find the distance when the angle HSP is acute.

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In the second case,

1. $2c : 2b - 2a :: 2b : \dfrac{2bb - 2ab}{c} = \text{FX}.$

2. $c - \dfrac{bb - ab}{c} = \dfrac{cc - bb - ab}{c} = \text{SV}$ in schem.

3. $sa = \dfrac{cc - bb - ab}{c}.$

4. $csa = cc - bb - ab.$

5. $csa + ba = bb - cc.$

6. $\dfrac{bb - cc}{cs + b} = a$ [e]: which is the other theorem to find the distance when the angle HSP is obtuse.

The next thing that may be scrupled at, is that, in the construction of the problem, I say the hyperbolas, whose foci are A, B and B, C, (Plate 2. Fig. 5.) and transverse diameters SA − SB and SB − SC, do intersect one another in the focus F: to evince which I say that from the notion of an ellipsis SB + FB = SA + FA, and, by transposing the parts of the equation, it is SA − SB = FB − FA; so that although [neither] FB nor FA be known, yet their difference is given, being = SA − SB; and seeing it is the nature of an hyperbola to have any two lines, from its foci to any point in its curve, always differing by the transverse diameter, it follows that the point F is somewhere in the curve of an hyperbola, whose diameter is SA − SB and foci A, B. And by the same means we may prove the point F in the hyperbola, whose diameter is SB − SC and foci B, C. Now these two hyperbolas can intersect one another but in one point, which is the

[e] ab, in the right hand of equation (2), becomes positive: whence (6) would bring out $\dfrac{bb-cc}{cs-b} = a$; but Halley mentions at the end of his letter that it was written in great haste. (2) ought to be $\dfrac{bb - ab}{c} - c$, which gives the true value of a.

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focus F required, which found, if we draw the line FB, FC, or FA, the sum of any one of them and the line from S to the same point, is equal to the transverse diameter.

The theorem I give for finding the square of the third side of a triangle, having the angle comprehended between two given sides, is nothing but an easy corollary from the 12th of the 2d of Euclid, and is there demonstrated. I desire you would please to note as much in my paper.

The last thing that may require the reader's extraordinary attention is that the square of DG is equal to the square of FB, or *a*, in the square of the sine of the angle DBG. The demonstration is founded upon the 20th of the 3d of Euclid; for from it, it is easily proved that the quadrilaterum BDFG is in circulo, whose diameter is *a* or FB, as also that DG is the subtense of double the angle DBG. Now the diameter is to the subtense of the double arch as the radius to the sine of the single, the proportion being as 2 to 1; wherefore as FB, diameter, to DG, the subtense of the doubled angle of DBG, so is radius to sine of DBG. Wherefore the rectangle of the sine of DBG and FB is equal to the rectangle of DG in radius; but radius being unity, (neither multiplying nor dividing,) the sine of DBG and FB, or *a*, is equal to DG. And therefore the rectangle of the squares of that sine and FB is equal to the square of DG: quod erat demons.

The example of the calculus.

Log. SA = 46956
SB = 40361
SC = 32297
The angle ASB = 67° 20′ 00″
BSC = 67 40 40

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By which data SA − SB = b = 6595
and by trigonometry AB = c = 48715.27
SB − SC = d = 8065
BC = f = 41010.91
and the angle ABC = 109° 33′ 51″
its logarith. sine = 9.9741740
cosine = 9.5248663

From which data it will be easily found that the logarithm of $\frac{cc-bb}{2c} = g$ is 4.3786017

and of $\frac{b}{c} = h$ = 9.1315497

whence 9.1315497 a + 4.3786017[f] = DB in schemate.

Likewise that [of] $\frac{ff-dd}{2f} = f$ = 4.2947447

and [of] $\frac{d}{f} = l$ = 9.2936510

whence 4.2947447 − 9.2936510 a = BG in schem.

From whence it is easy to deduce, according to the theorem[g] I gave in my paper, that

$$SSaa = \left\{\begin{matrix} +hh \\ +ll \\ -2hls \end{matrix}\right\} aa \quad \left.\begin{matrix} +2gh \\ -2kl \\ -2gls \\ +2khs \end{matrix}\right\} a \quad \begin{matrix} +gg \\ +kk \\ +2gks \end{matrix}.$$

[f] The natural numbers belonging to these logarithms must be supposed to be taken before the addition is made in this equation: the same applies to the expression for BG.

[g] The expression here alluded to will be found at page 240, and when its terms are arranged according to the powers of a, it takes the form, which gives the value of $SSaa$.

Halley annexes each of these quantities to the logarithm which in the present case belongs to it: for example, he writes

$$\left\{\begin{matrix} +8.2630994\ aa = hhaa \\ +8.5873020\ aa = llaa \\ -8.2510970\ aa = 2hlsaa. \end{matrix}\right.$$

This would have made the whole

[and the logarithms of these respective coefficients will be]

$$9.9483480\,aa = \left\{\begin{matrix}8.2630994\\8.5873020\\8.2510970\end{matrix}\right\}aa \quad \left.\begin{matrix}3.8111815\\3.8894257\\3.4981490\\3.2521907\end{matrix}\right\}a \quad \begin{matrix}8.7572034\\8.5894896\\8.4992429\end{matrix}$$

which equation, in natural numbers, is

$$0.88786720\,aa = 0.03916315\,aa - 2639.644a + 1276012490;$$

which reduced is,

$$1276012490 = 0.84870405\,aa + 2639.644\,a;$$

in which equation the root $a = \text{FB}$ will be found 37251, to which if you add SB = 40361, the sum is 77612, whose half is 38806, agreeing to a unit with the mean distance of Mercury from the sun, determined by Kepler in his Rudolphine Tables, from which I took the distances and angles in the example.

Thus I hope I have cleared those doubts, which may be thought considerable, in that little treatise I gave you. In it I affected brevity as much as possible, as knowing that it most respected the learned in astronomy; and to all such I doubt not, but what I here send you will be a sufficient demonstration. But to those that have not examined deeper into the fundamental of the science, it would be an endless piece of labour to make it intelligible. I must beg your excuse for the many faults, I doubt not but I have committed in this long letter, being straitened of time, of which I have not so much as to read it over. Yet I have been so careful of the numbers and sym-

too large for the page: and it was likewise thought that the separation would render the reasoning more clear, by disuniting the logarithms from the symbols + and −, which really belong, not to them, but to the natural numbers, which they designate.

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bols, that I dare promise them true. I can add no more, but that I am

Your most obliged friend and devoted servant,

Queen's Coll. EDMOND HALLEY.

I could not delineate the schemes requisite at this time, but I will send them by the next post.

This letter is made up, as it is now printed, from two copies; the one written on four sides of foolscap complete, and the other, containing the two last pages enlarged, by additional matter, into three. This last is a part of what was actually sent to Oldenburg: it has on the back of it a memorandum in his handwriting, "Rec. July 10, 76;" and the identical two first pages, which are wanting in Lord Macclesfield's collection, are preserved in one of the guard books belonging to the Royal Society. Their substance, with some subordinate and unimportant variations, answers to the beginning of the letter as it is here printed. This first part extends to p. 229, line 18.

LXXXIV.

HALLEY TO OLDENBURG.

Sir, Oxford, July 11, 1676.

I have here sent you the two schemes, (Pl. 2. Fig. 4,5)[h] to which the demonstrations of my former letter refer. They ought to have been sent in that; but the post going out so early as he does, gave me no time to

[h] The letters H, E, K, L, (on fig. 5.) are not the same as those attached to these points, on the manuscript diagram. The alteration was made to facilitate the reference to the paper, as it is printed in the Phil. Trans. (vol. XI. p. 683.) HE = FB — FA, the transverse axis of the hyperbola, whose foci are A and B; and KL = FC — FB, the transverse axis of the hyperbola, whose foci are A and C.

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delineate them; but now I hope they will come to you soon enough. If you shew the latter part of my letter to my Lord Brounker, I must entreat you to excuse to him the coarseness of my lines, and imperfection of the demonstrations, if any be, by the haste I was in when I wrote them. I am now in the same case, and have only time to delineate the schemes, and subscribe myself

Your most obliged friend and humble servant,

EDMOND HALLEY.

To these two letters is attached the first draught of Halley's paper, which is here annexed, not only from the interest which it possesses in itself, but as it elucidates the correspondence.

A direct Geometrical Process to find the Aphelion, Eccentricities, and Proportions of the Orbs of the Primary Planets, without the Supposition, hitherto employed, of the Equality of the Motion at the other Focus of the Ellipsis [i].

The motion of the earth annually through the ecliptic causes an apparent inequality in the motions of the other planets, which is known to astronomers by the name of the parallax of the orb, and is the basis of the ensuing method, to which I suppose the orbs of the planets to be ellipses, and that the sun is constituted in one focus common to them all, and that we know the times of their periods at least so well that there shall be no sensible error in two or three revolutions. Upon these suppositions the earth's motion (being the key to all the rest) is first to be sought.

[i] In the margin Oldenburg has written, "Compare this with No. 57. of the Transact." which contains, at p. 1168, N. Mercator's Considerations on M. Cassini's method of finding the apogees, eccentricities, and anomalies of the planets.

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In Pl. 2. fig. 6[k], let S be the sun, ABCDE the orb of the earth, P the planet ♂, (who, to this purpose, is for several reasons much better than any other,) let the true time and place of the opposition of the sun and Mars be exactly observed, then is the sun and earth in a right line with Mars, or, if he have latitude, with the point, whence the perpendicular from him falls upon the plane of the ecliptic as in the figure: S, A and P are in a right line. Again, after 687 days, Mars returns to the same point where he was before in opposition to the sun; but the earth, not returning to A till after 730 days, is short of the former place, and beholds the sun in the line SB, but Mars in the line PB; and by observing the longitudes of the sun and Mars, all the angles of the triangle PSB are given; and supposing PS 100000, in such parts we may find the length of the line SB. So, after another period, the earth being in C, we may find the line SC, and by the same means the lines SD, SE; and the differences of the sun's observed places are the angles ASB, BSC, CSD, DSE: so that it comes to this geometrical problem, viz. three lines from one focus of an ellipsis being given, both in length and position, to find the length and position of the transverse diameter, with the distance of the foci. The resolution of which extends itself to all the rest of the planets, if, after the theory of the earth's motion be known, we find, by the method proposed by my Lord Bishop of Sarum, in his Astronomia Geometrica, lib. ii. part ii. cap. 5, three distances from the sun with their positions. But in regard his Lordship supposes, that the planet observes an equal motion at the other focus of the ellipsis, and thereon grounds his calculus, I think it not amiss to

[k] The figures 6 and 7 are wanting in the manuscript.

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shew how the same thing may be done without that supposition, which to lay aside observation gives us good reason.

In Pl. 2. fig. 7, let S be the sun, 1A2B the orbit of the earth, P the place or point in the plane of the orbit of the earth, where the perpendicular from the planet falls, AB the linea apsidum of the orb of the earth. Let the planet in P be observed first from the earth at 1, and, after a period of the same planet, let the earth be in 2, and, as in the former place, let the sun and planet's longitude be observed. By the longitudes of the sun and place of the aphelion, the angles AS1, AS2, are given, and consequently the sides S2, S1, are found. For if the angle of true anomaly be acute, the proportion is as the difference of the mean distance, and [the] cosine of the angle multiplied in the eccentricity, is to the aphelion distance, so [is] the perihelion distance to the distance of the planet from the sun at that anomaly: but if the angle be obtuse, the first term of the proportion is the sum of the two parts, whereof the first term of the former is the difference. Then in the triangle 2S1 are given 2S, 1S, 2S1; and 21, S12, S21 are required: again, in the triangle 2P1 are given 21, 2P1, 21P; required 2P: and in the triangle 2SP, 2P, 2S, S2P are given; and SP, 2SP required: then as SP to 2P, so tangent of the visible latitude to tangent of inclination or latitude at the sun; and as cosine of inclination to radius, so SP, the curtate distance, to the true distance of the planet from the sun: so that we have found the position and length desired. Now it remains to shew, how by any three of these lines, and the angles intercepted, we may find the transverse diameter and eccentricity of the ellipsis.

In Pl. 2. fig. 5, let SA, SB, SC, be the three distances in their due position, and drawing AB, BC, let AB be the

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distance of the foci of an hyperbola, and SA − SB its transverse diameter, by which foci and diameter describe that hyperbolic line, whose vertex is nearest to A, the extremity of the longer line SA: so likewise let BC be the distance of the foci of another hyperbola, whose diameter is SB − SC, and describe that hyperbola, whose vertex is the extremity of the diameter next B, I say these two hyperbolas thus described intersect one another in the point F, which is the other focus of the ellipsis, and drawing the lines FA, FB, FC, SA + FA, SB + FB, or SC + FC, is equal to the transverse diameter. Then having the transverse diameter and foci, the description of the ellipsis is most facile.

But to perform the same arithmetically, let us suppose it done, and let $FB = a$, $SA - SB = FB - FA = b$, $AB = c$, $SB - SC = FC - FB = d$, $BC = f$, sinus of the angle ABC $= S$, cosinus of the same $= s$. Then as $c : b :: 2a - b : \frac{2ab - bb}{c}$, and $\frac{2ab - bb + cc}{2c} = DB$, per 36. 3. Eucl. and $f : d :: 2a + d : \frac{2ad + dd}{f}$, and $\frac{ff - 2ad - dd}{2f} = BG$ per eandem; and for ease of the calculus, let $\frac{cc - bb}{2c} = g$ and $\frac{b}{c} = h$, likewise let $\frac{ff - dd}{b} = k$, and $\frac{d}{f} = l$, then will $BD = g + ha$, and $BG = k - la$. And because that in all obtuse-angled triangles the square of the base is greater than the sum of the squares of the sides by the double rectangle of the sides in the cosine of the comprehended angle, it follows that $gg + 2gha + hhaa + kk - 2kla + llaa + 2gks - 2glsa + 2khsa - 2hlsaa =$ to the square of DG; but DG is equal to the sine of the angle DFG or DBG multiplied in a; so that $SSaa = gg + 2gha + hhaa + kk - 2kla + llaa + 2gks - 2glsa + 2khsa - 2hlsaa$,

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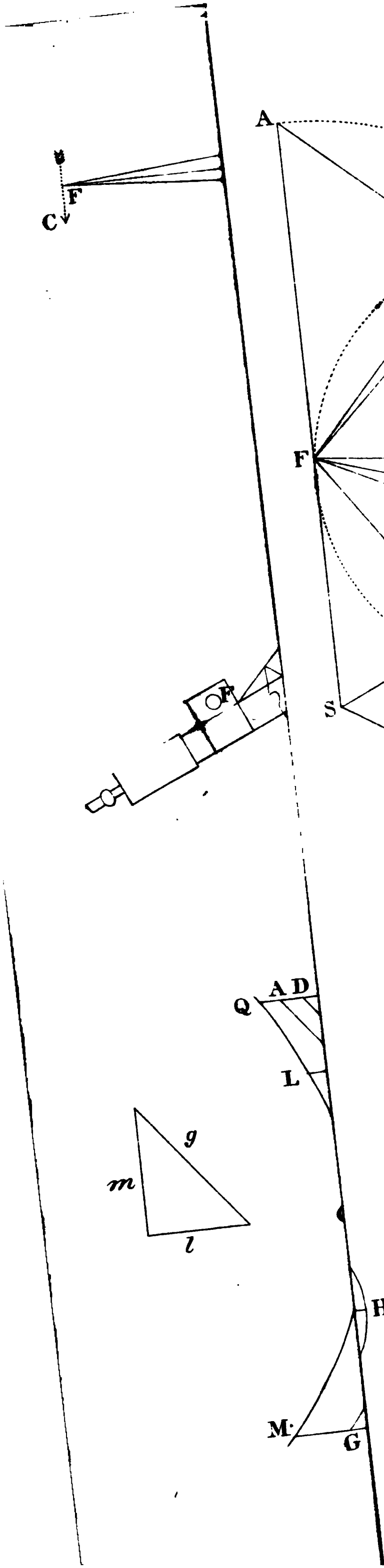


Fig. 5. *p. 235, 9*

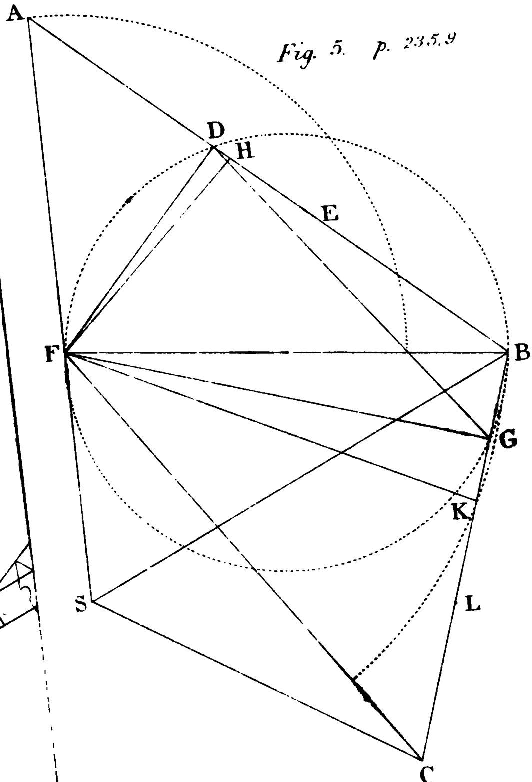
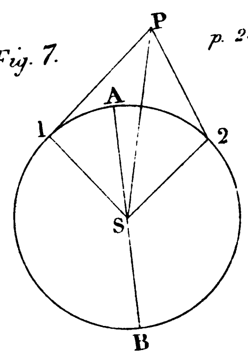


Fig. 7. *p. 239.*



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which equation is easily resolvable, being no higher than a quadratic affected.

By all which it is apparent that my Lord of Sarum might have spared two of the observations, he was pleased to make use of, in the resolution of this problem, in lib. i. part ii. cap. 10. of his Astronomia Geometrica, where he wholly neglects the limitation given by the point S being the focus of the ellipse, and makes use only of the defect of the ellipse from the circle, which in most of the planets is hardly sensible, and therefore the determination therefrom must necessarily be very uncertain.

Against the end Oldenburg has written the following memorandum:

This paragraph may be omitted, not to give offence to a person in so eminent a station: the intelligent reader being like to find out thus much himself, by comparing both.

Halley was an undergraduate at college, not twenty years of age, when he drew up this paper, which is the first specimen which was given to the world of his powerful talents. The exposition of his method is not dated, but it was most probably communicated to Oldenburg in the spring or summer of 1676; because the demonstrations, when called for, were written out without loss of time, and dispatched to London in the beginning of July. The following letter, which he afterwards wrote to Oldenburg, is preserved in one of the guard books of the Royal Society, and bears so immediately upon that which is the subject of the present remarks, that it seems worth while in this place to insert the whole of it.

Aug. 8°. 1676.

Sir, Queen's College, Oxford.

I have yours of the 15° of July, and my papers which you sent in it. I had sooner returned them again to you, had I

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not been hindered by some exercise I was obliged to do in the house, for my degree, and if you had not allowed me so much time as till the publication of your August Transaction[1]: but as soon as I saw that of the last month, I set to it, and have herein sent it you, and, by the advice of some friends, in Latin; that language having been all along made use of, in your Transactions, in astronomical matters. I think I have somewhat mended the composure of the treatise, and inserted the demonstration in short, so that I hope I may receive no discredit, but rather the contrary, from the publication of it: and as to the schemes, I have sent them drawn according to your directions, but that sort of ink I find very unfit to draw fine strokes, being very apt to clot in the pen. The two first schemes may be better (as I think) done by the graver himself, there being nothing required but two circles and straight lines drawn ad libitum; and if they be but like them I send, it is sufficient; and if these be too big, he may at pleasure make them smaller, but the third cannot well be less without confounding the lines and letters, which seem, as it is, to be somewhat too small. I could wish you had informed me, to whom you had shewed my paper: I guess it to be Mr. Collins. I hope by your next you will let me know.

I pray, Sir, signify the receipt of this by a line or two, and let me understand what you hear from France about the southern stars, which in your last you promised me to inquire into, and if here there be any appearance of encouragement for my friend that will go along with me. I design to be in London within one month to fit instruments and necessaries for my intended voyage, in which I promise myself that I shall be able to do something acceptable to the learned world. I hope, by that time, you will have done my small treatise the honour of publication in your next Transaction, for which I shall not be wanting to seek all opportunities to express how much I am, Sir,

Your most obliged friend and servant,

EDM. HALLEY.

[1] The paper was printed in the 128th number of the Philosophical Transactions, which is dated "Sept. 25, 1676, for the months of August and September."

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Upon this Oldenburg has written, "Rec. Aug. 9, 76: ans. Aug. 10, 76." Halley embarked for St. Helena in the following November.

LXXXV.

COLLINS TO OLDENBURG.

FOR LEIBNITZ.

As to the solving of all equations by the table of sines, that it might be done hath been often asserted by Dr. Pell; and lately in the hearing of you yourself, (Mr. Oldenburg,) being asked whether he could solve all equations of six or eight dimensions by the canon of sines, he asserted it, affirming he had reduced equations of higher dimensions to the said canon; wherefore be pleased to report it on account of your own knowledge, lest coming from me only it should seem incredible; and for me that am ignorant to give an idea of it, will be somewhat difficult; but however I shall endeavour it as well as I can; and,

1. The said Doctor affirms the doctrine of angular sections may be infinitely enlarged, which seems true by a specimen at the end of the High Dutch Algebra of his scholar Rhonius, where there are 105 theorems about sines, chords, tangents, and secants, which are not printed in the English edition.

2. That the chief end and benefit of this doctrine is not so much the making of the tables, (which may be more easily done otherwise,) as the resolution of equations.

3. That the circle and ellipsis, with their inscripts and adscripts, are more proper for this purpose than any other figures: for instance, in Mr. Gregory's Geo-

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metriæ pars universalis, occurs this proposition, page 128:

Si circuli circumferentia dividatur in partes quotcunque æquales et numero impares, et a quolibet peripheriæ puncto ad omnes ejusdem divisiones rectæ ducantur, si circulus dividatur in tres partes æquales, erit summa primarum æqualis ultimæ; si in quinque, erit summa primarum et ultimæ æqualis summæ secundarum; si in septem, erit summa primarum et tertiarum æqualis summæ secundarum et ultimæ; si in novem, erit summa primarum tertiarum et ultimæ æqualis summæ secundarum et quartarum; atque ita deinceps in infinitum. Dicimus autem rectas primas esse illas, quæ ducuntur ad divisiones, ex utraque parte, puncto assignato proximas; secundas illas rectas, quæ ducuntur ad divisiones primis, ex utraque parte, succedentes; tertias, quæ secundis succedunt, &c.: rectam vero ultimam illam quæ ducitur ad divisionem a puncto assignato remotissimam.

4. The like Dr. Wallis hath done when the periphery is divided into any number of equal parts, and hath given the equations proper in order to obtain the same, printed in a Treatise of Angular Sections remaining in my hands.

5. These chords, representing the roots of equations, may be transferred from the circle as ordinates standing upon their proper resolvends, through the tops of which ordinates a curve being drawn, shall be flexuous or indented, as the loca of all equations are, as hath often formerly been hinted, and is evidently already known in cubics; and from hence some light may be gathered for transferring back from the locus to the circle.

6. Dr. Pell affirms that he can constitute problems

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that shall come to an equation of the like form with any proposed.

7. That in such constitutions he can attain the limits ascendendo.

8. That the doctrine of limits is yet, even by the best that ever writ, but very imperfectly handled.

9. That by comparing or adjusting the limits of equations and problems together, those rules of Cardan, and infinite others like them, may be found; and that rule or doctrine of Hudden, of finding the surd roots of all numeral and literal equations, be attained.

10. The limits being obtained, for the avoiding that intricate complication of surds, he useth the canon, which may likewise be done in the attainment of the very limits, the which being once had, the finding of all the roots, to any resolvend proposed, is done by an easy method of applying the said resolvend to one circle, or divers of the resolvends to several circles, each of which may be conceived to have several revolutions.

11. That he hath written exercises of this doctrine long since, entitled Tractatus de habitudinibus repetitis et usu canonis mathematici, but his papers are in the country.

12. These assertions might beget a jealousy in a student's thoughts: first, whether there be not a possibility of increasing, diminishing, multiplying, and dividing some of the roots of an equation, keeping all the rest constant; and secondly, whether if two equations having all the same pairs of equal roots, saving only one pair in both, common, what habitudes or variations between the roots in each, and between how many of them. Thirdly, it seems probable that each pair of roots in every high equation may have several

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canons for finding them: as for instance, Cardan's rules will find the root of a cubic equation, when there is but one root possible, and upon new forming [and] decreasing by the root of the limits that the penultimate term may vanish and the second come in in the room of it, there may be other canons for finding the roots when there are three possible.

13. Upon the account of these attainments Dr. Pell long since, in his Idea of Mathematics, reprinted here in a little book of Hartlib's, in anno 1651, in 12°, in English, for Robert Littlebury, entitled The Reformed School, proposed or promised, viz.

According to his described method (p. 43.) to deduce not only all that ever is to be found in our antecessor's writings, and whatsoever they may seem to have thought on, but also all the mathematical inventions, theorems, problems, and precepts that it is possible for the working wits of our successors to light upon, and that in one certain unchanged order, from the first seeds of the mathematics to their highest and noblest applications, as well as to the meanest and most ordinary, not setting them down at random as they come in my head, as those before us have done, so that they seem to have lighted upon their problems and the solutions of them by chance, and not to have found them by one perpetual, constant, invariable process of art: and in p. 45, any subject being propounded, to determine the number of all the problems that can be conceived concerning it; and any problem being propounded, demonstratively to shew, either all the means of its solution, or the impossibility of it; and if so, then whether it be not yet, or not at all possible, he hath writ an exercise on this argument, entitled Cribrum Eratosthenis, of which Mr. Boyle hath had the view;

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and 'tis his common assertion that all that hitherto have writ of Algebra, do not sweep clean, or leave large gleanings after their harvest.

14. The said Doctor, being censorious of others, and incommunicative, himself declining discourses about his methods, was at last censured by Des Cartes for those assertions, concerning whom the Doctor never had any extraordinary esteem. And those letters or censures of Des Cartes, one Mr. Haak, an aged Dutchman, here, a member of the Royal Society, who translated the Dutch annotations on the Bible into English, hath a copy of, but, upon the account of his friendship to Dr. Pell, will not impart. We earnestly desire, if they can be procured from M. Clerselier, or any other person, to vouchsafe transcripts thereof.

15. To render all credible, take this narrative: Dr. Pell, in divers companies, who can attest the same, took out of his letter-case a scroll of paper of about a yard long, ruled with many columns, wherein against, as I believe, four hundred resolvends, arithmetically increasing, of an equation of six dimensions, (as I remember,) was writ down in several columns the several series of roots belonging thereto, which he asserted were taken from a table of sines, and yet the equation was not proper to angular sections. He said moreover, that for the better doing this work, it was necessary there should be a new large canon made, dividing each degree of arch into a thousand parts; to which 'twas replied, that when the benefit of it was understood, the canon might possibly be undertaken, by aid of which table of roots, he had accurately described the locus of the equation with all its flexures, shewing where the roots gained or lost their possibility by pairs; that these series of roots were almost as easy to make as transcribe; that to attempt the same

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in Victa's method, Mr. Warner used to call work unfit for a Christian, and more proper to one that can undertake to remove the Italian Alps into England.

16. Upon conference with the Doctor I do not find that he is knowing in the doctrine of infinite series; and though he grants they may be of good use, as to the theorems or rather habitudes thereby invented, yet as to the calculative or applicatory part, he says it may be either quite removed or exceedingly facilitated by his methods, which he will forbear to make common, till he first sees what will come out of Mr. Gregory's and Mr. Newton's, the latter having read lectures thereof and of algebra, and put them into the public library at Cambridge.

17. These things are worthy the contemplation of the learned at Paris, and I hope they will impart an account of their endeavours and success. Be pleased to inform M. Leibnitz that I have a friend, namely, Mr. John Smith, a musician, now at Lyon, on his return from Rome, who hath procured such new books, as the Rev. Pere Alphonso Borelli could advise him to. I have writ to the said Mr. Smith to attend the said M. Leibnitz, and crave his assistance in the procuring Frenicle's book des Triangles Rectangles, de la Hire's little tract of the Conic Sections, or any other new thing he shall advise.

LXXXVI.

ADR. VERWER TO D. GREGORY.

Literas tuas, ad me datas VIII Id. Februar. MDCCI, tempore debito accepi; sed quod non responderim prius gravis ponderis habui impedimenta. Dominus

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Gulielmus Moncrief, civitatem sane transiens ad te, me monebat nonne literularum quidquam, tibi tradendum, ei committerem. Quare diutius expectare nolebam, sed raptim scribo.

Quæ de opere tuo astronomico attingis, rite ac mature examinavi; placent et mihi et aliis. Mecum fuerat paulo ante communicatus a D°. Stratford index ejus operis, typis expressus, una cum stipulatione de subscribendo. Et causa tua eo in pretio apud me fuit protinus, ut stipulationem eam aliquoties fecerim publicari in Hollandicis relationibus publicis, (vulgo Les Gazettes,) meis impensis: quo hic et in Germania vicina de eo opere innotesceret fama, scirentque eruditi quo sese possent conferre ut copiam ejus possent nancisci. Indicavit nunc porro Dominus Moncrief opus illud prodiisse in lucem, et distracta esse exemplaria, ducentis excerptis, quæ quidem nunc permutare satagis cum nostris bibliopolis Hollandicis. Contractui ejusmodi non deerit occasio: verum requirunt hic visum exemplarium, saltem duorum, (et charta majori et charta communi). Quare autor tibi sim ut exemplarium decadem transmittas, prima data occasione, Domino Joanni Drummond; et tunc ille mecum una omnem operam impendet ad contrahendam permutationem, et in antecessum conditiones, librosque a parte hac tradendos. Tibi designabimus quemadmodum et tu scribere mihi potes quosnam desiderares libros, ut ne tempus abeat incassum. Quantum hic tibi valere possint mea officia, lubens id omne tibi offero.

Arithmetica Astronomia, quæ a tractatu tuo aberit, ut videtur, postea etiam absque dubio e penu tuo nobis proveniet. Eam etenim desideramus maxime. Philippus de la Hire, partem eam calculatricem, non exhibuit nisi quoad solem et lunam. Verum dignissima sane res esset habere eandem quoad errones, eorumque

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satellites. Ratio quare Philippus de la Hire hanc ultimam non vulgaverit ads .... ea est, quod eatenus non pauca possit dici desumpsisse ... e penu Domini Cassini, et ideo, quamdiu vixeret Cas[sinus] publicationem differre proposuit.

Argumenta, quæ in opere tuo tradenda sumis, sunt in illo genere omnium dignissima; et præterea erunt a te pro dignitate sua exculta, ut videre aveo.

De Cl. Viviano etiam nihil accepi, nisi ante biennium; sed ait Dominus Moncrief nunc obiisse diem, post mensem unum aut alterum, Florentiam. Perscribam amico cuidam, qui certiorem me faciet. Synthesis P. Guidonis Grandii, Vivianeorum problematum excellit multis nominibus. Erit illa tibi perlata. Optandum sane ab illo Grandio plura prodire.

Quid apud vos sub prelo jaceat observo; Dominus Stratford videnda mihi præbuit specimina aliqua foliorum et placent admodum. Euclides, Apollonius grata erunt volumina.

Observo itidem quæ scribis de itinere Domini Halleii. Forsan ea etiam videbunt lucem, ut a nobis possint legi.

Hic apud nos silet exercitium omne in re mathematica, neque ullus est qui proferat quidquam. Ego horam subsecivam libro Newtoni impendo; nam familiarem eum mihi feci præcipua ex parte, quoad ea nempe, quæ viam aperire debent ad necessarium ejus intellectum: et mirabile viri illius ingenium obstupesco indies.

Tædet me quod tantus non es in lingua Batava, ut libellum meum Inleiding toe de Christelyke Godsgeleerheid intelligas. Quapropter præcipuum ejus argumentum breviter hic tibi impertiam.

Statuunt theologi tria Servatoris nostri munera, propheticum, sacerdotale, regium; tunc propheticum

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esse totum in docendo et prædicando τὰ cultus divini στοιχεῖα, Dei videlicet existentiam et attributa; item seminans officia practica.

Istud munus propheticum proprie in libello meo perstrinxi in gratiam liberorum meorum, mero quidem ratiocinio et geometrico ordine. Scilicet docuit me peritia vulgares theologos, ut plurimum, consumi vel in sectæ studio vel in crassa ignorantia. Liberos meos volui θεολογίζειν ratione duce; et ne in errores ruerent, iis relinquere fuit animus quicquid ego de hac materia sane essem meditatus. Et in scholiis adjunctis monstro convenientiam meæ conclusionis, per viam ratiocinii, cum effatis S. Scripturæ, id quod omne est punctum. Et primum pro firmanda existentia motoris Dei, adduco in argumentis non motum localem in universum, sed motus speciem ellipticam; quippe quæ sola obtinet in omni phænomeno cælesti, neque absque motoris manu considerari potest effectu possibilis. Ex sola Dei existentia eruo existentiam beatitudinis æternæ: quod me ante nemo fecerit. Invenies quidem Oxonii Hollandum aliquem, qui tibi argumenta libelli explicaverit. Tunc de officiis practicis, ad beatitudinem æternam proportionatis, quædam firmo: ac tandem in schol. 2. post Propos. IV. res mihi deducitur ad veritatem, quæ in tota re theologica est plane palmaria; eam nempe quam S. pagina sic profert: "Sanguis J. Christi mundat nos ab omni peccato: modo adsit conditio hæc: si ambulemus in luce." 1 Joann. i. 7. id est si ambulemus in bonis operibus. Ephes. ii. 10: adde Coloss. i. 10–14. Clariore phrasi dicendum beatitudinem æternam reapse adquiri per opera pia, et gratiam divinam conjunctim: et quidem ita ut operum piorum defectus aut imbecillitas per gratiam suppleatur. Rom. viii. 3, 4.

Ut nunc reliquum libelli contexerem per naturale

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ratiocinium, modo qui certus esset ac infallibilis et nihil haberet superflui, placuit ad veritatem eam acquisitam mihimet meditari analysim mathematicam. Observaveram dudum id cum fructu factitatum a Pitcairnio in tractatu de Inventoribus. Eam igitur veritatem dialecto Newtoniana ita efferebam. Beatitudo æterna est in ratione composita ex operum piorum ratione directe et ex gratia divina ratione reciproce: et pro æquatione producenda exprimebam per D nomen Dei; f = felicitas Dei; h = homo; m = medium; e = finis; G = gratia. Unde ex hisce datis est $\frac{Dh}{f}$ = felicitas æterna hominum: est quippe status, qui est ad h ut D ad f. Item $\frac{Dhm}{ef}$ = opera pia; sunt enim ad felicitatem æternam hominum ut est m ad e.

Veritas præfata igitur analytice sic sonat.

$$\frac{Dh}{f} \text{ ut } \frac{Dhm}{ef} \times G.$$

qua ratione posita æqualitatis, post reductionem debitam, exsurgit æquatio hæc, $D \times efh - D \times fhm \times Geff = 0$, ipsaque est facies, qua æquationem in libello meo vulgavi.

En porro quo me juverit ejusmodi analysis.

1°. Possit f esse sic indeterminatam, patet æquationem esse dimensionis unius. 2°. Quoad limites meæ æquationis: ubi quantitates D, f, h, G, item ratio m ad e, aut unaquæque earum fiant nihilo æquales, destruitur æquatio, unde elicui unamquamque earum quantitatum esse conditionem æquationis: id quod innui disertis propositionibus,

quoad Deum, et beatitudinem æternam Prop. I.
quoad Hominem pag. 16. lin. 15. item Prop. IV.
quoad Gratiam Prop. IV. Schol. 2.
quoad Opera Prop. IV. V. VI.

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Ex operum ratione *m* et *e* sequitur Prop. VII. item VIII. IX. X. XI. XII.

Prop. XIII. est omnium præcedentium corollarium.

Casus porro æquationis sunt si,

Ex natura rationis inversæ et reciprocæ, in qua sunt inter se opera pia et gratia, fluit quoties differentia inter terminos *m* et *e* est infinite magna, etiam gratiam in infinitum excrescere et tunc jure merito dici satisfactionem. In omni alia differentia gratia est suppletoria.

Atque ita observabis, vir clarissime, me per viam talem analyticam omnes meas conclusiones tuto potuisse perficere, nihil affirmando aut negando quod non præscriberent termini, conditiones et casus æquationis.

Vellem et Newtonum hæc posse persentire. Moncriefio trado exemplar ei perferendum, ut et D. Thomæ [Gilberto] Burnet episcopo; et sic [hic?] linguam Belgicam satis callet.

Neque aliud habeo, quod addam, commendans me tuæ amicitiæ et bene valere te jubens.

Dabam Amstelædami xiii Kalend. Februar. MDCCIII stylo Gregoriano.

LXXXVII.

WILLIAM ARCHBISHOP OF DUBLIN TO THE BISHOP OF CLOGHER.

My Lord,

I send you, as I promised, the phases of the sun and moon as taken under the eclipse. There is an equal number of observations, but not exactly at the same times with what I sent before. This was taken by Mr. Hawkins, the king at arms, who has a very good eye and hand, and is a man curious both in his observations and draughts.

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I send you likewise the former, with the time as calculated from the observed altitudes of the sun. By this you'll observe the several differences between the clock-time and calculated time from the altitudes, and by those two one may pretty justly guess, concerning the exactness of the observations, and where the errors lay. From the whole the observer concludes that the latitude of the observatory at St. Sepulchre's, Dublin, is 53° 19′ 46″ 34‴. The usual latitude of Dublin is reckoned 53° 20′. The difference is 13″ 26‴, which doth not make a quarter of a mile; and if the observation made by Dr. Sommers, that fixed the latitude of Dublin at 53° 20′, was made in the college, as it really was, this agrees exactly with it, being made a quarter of a mile more southerly.

How far this may help to fix our longitude in respect of London I can't say, but I believe it may be useful to that purpose; and, as soon as the several observations are printed, I will take care to have the calculation made.

As to the pale light about the moon in the greatest obscurity, I believe it can hardly be otherwise accounted for than by an atmosphere: but whether of the sun or moon, or both, will require thought to determine. By the increase and decrease of the moon we perceive that there are protuberances in her. If one could have observed any thing of these in the eclipse, and whether the pale light was stronger in the lower parts than there, it would help to fix the atmosphere to the moon. For if there be any thick air, 'tis certainly most in the valleys.

The time of the eclipse was deadly cold here. Many took colds in it, and there was a very great dew. Every thing looked with a yellow aspect inclining to green, &c. WILL. DUBLIN.

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This letter is printed from a copy made by sir I. Newton. It was most probably written by Dr. William King, who was Archbishop of Dublin from 1702 to 1729.

LXXXVIII.

FROM MACHIN.

Sir, July 23, 1706.

In answer to your request I looked in the minute book to see what the parallelism was for the 31st Aug. and the 21st Sept. 1705; but can find nothing set down for it; which I believe might be the reason, that he has not copied the observations of the moon as well as Venus about those times, but left them blank.

The parallelism for the 21st July was .19 to be added;

The next parall. is for the 12th Sept., which is .54 to be added;

The parall. for the 2d October is .44 to be added.

I have however sent you the copy of the distances you required, and allowed .50 for both times, which, if there was any regularity in the increase or decrease, seems to be what it might have been.

Aug. 31, 1685.

Corrected time. h.	m.	s.		°	′	″	Revol.	°	′	″
1	15	44	♀ a vertice	54	3	52	490.59	3	47	6
	30	51	♀ a limbo ⊙ remot.	20	27	25	489.85	20	27	21
	33	16	.... rep.	20	27	20	489.82	20	27	16
	35	35	♀ a limbo ⊙ prox.	19	55	5	477.04	19	55	7
	38	17	.... rep.	19	55	5	477.04	19	55	7

Sept. 21, 1685.

I allow 14′ to be added for the error of the clock, as I suppose it was by the succeeding days.

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Corrected time.

h.	m.	s.		°	′	″	Revol.	°	′	″
1	50		♀ a vertice	64	26	07	107.33	4	30	21
2	30	0	♀ a limbo ⊙ remot.	25	41	47	615.22	25	41	54
	31	26	...............rep.	25	41	57	615.32	25	42	9
	34	55	♀ a limbo ⊙ prox.	25	9	57	602.47	25	9	55
	37	29	...............rep.	29	10	7	602.55	25	10	7

And this being the best account I can give you, I take leave to subscribe myself

Your obliged humble servant,

JOHN MACHIN.

This letter probably was addressed to Halley, and refers to the observations in p. 181 of the first volume of the Historia Cœlestis. The objects are there mentioned, without any numbers annexed to them; and this letter may have been occasioned by Flamsteed having omitted them in his transcript.

LXXXIX.

MONTMORT TO JONES.

à Paris, 16 Janv. 1709.

La haute idée, Monsieur, que m'ont donné de votre sçavoir géométrique les journalistes de Leipsic, dans l'excellent extrait qu'ils ont donné de votre ouvrage intitulé Synopsis Palmariorum Matheseos, m'a déterminé, quoique je n'aie pas l'honneur d'être connu de vous, à vous envoyer un petit ouvrage qui a pour titre Essai d'Analyse sur les jeux de hazard, et j'ai cru l'honorer en le mettant dans les mains d'un aussi habile homme que vous.

Monsieur l'Abbé Bignon, qui est président de notre Académie des Sciences veut bien se charger de vous faire tenir ma lettre et mon ouvrage. Voudriez vous bien, Monsieur, m'en accuser la réception et m'ap-

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prendre en même tems des nouvelles de l'état des sciences en Angleterre.

Je vous prie, Monsieur, d'ajouter à cette grâce celle de me croire, très-parfaitement,

votre très-humble et très-obéissant serviteur,

REMOND DE MONTMORT.

Ma demeure est rue et près Sainte Croix de la Bretonnerie à Paris.

XC.

COTES TO JONES.

Sir, Feb. 15, 1711.

I yesterday received your most valuable and acceptable gift, together with your very kind letter. I return you my hearty thanks for 'em both. Not having heard any thing of your book till I saw it, I received it with the additional pleasure of a surprise. You have highly obliged the mathematical part of the world by collecting into one volume those curious and useful treatises, which were before too much dispersed; but more especially by the publication of the Analysis per Æquationes Infinitas, and the Methodus Differentialis. I could heartily wish that nothing of Sir Isaac's might be lost. I hope you will endeavour (as you find an opportunity) to persuade him to publish other papers; for I believe he has yet many excellent things in reserve. About a year and a half ago (when I was last in town) I acquainted Mr. Raphson that you had some papers of Sir Isaac's in your hands, which were long ago communicated to Mr. Collins. I thought they might have been pertinent to his design of writing a History of the Method of Fluxions. I afterwards understood that you gave him a sight of those papers,

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and that he thought 'em not to be for his purpose, which I do now very much wonder at, if his intention was to do justice to Sir Isaac. If that was not his intention, I think your Preface has already sufficiently defeated all his attempts. We are now at a stand as to Sir Isaac's Principia; he designs to make some few experiments before we proceed any further. The first book and the six first sections of the second are printed off. The inclosed paper is what I wrote about three years ago, and read to my auditors in our schools in 1709. I have sent it to you, as it relates to the Methodus Differentialis, but more especially as a small acknowledgment of my gratitude for having received that and the other excellent treatises from your hands, and as a token of my hearty friendship and sincere goodwill to you. I am, sir,

your most obliged friend,

and humble servant,

Cambridge, Trinity college. ROGER COTES.

This letter is printed in the Gen. Dict. vol. iv. p. 443.

XCI.

R. ALLIN TO J. WEEKES.

This is merely a note of thanks to Mr. Weekes, for having procured, for the writer, a present from Mr. Jones of the Analysis per Quantitatum Series, &c. It is dated from Sidney College, April 24, 1711.

XCII.

COTES TO JONES.

Sir, Cambridge, Sept. 30, 1711.

I return you my thanks for your letter, and the information you gave me concerning the state of the

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mathematics at present in London. I shall be glad to see M. de Moivre's Treatise of Chance when it comes out: his things are always very neat and curious. We have nothing of Sir I. Newton's, that I know of, in manuscript at Cambridge, besides the first draught of his Principia, as he read it in his lectures; his Algebra lectures, which are printed; and his Optic lectures, the substance of which is for the most part contained in his printed book, but with further improvements. I thank you for your kind offer of recommending my paper to the public; but I am of opinion that it is not of so great use as to deserve to be printed after Sir Isaac's Methodus Differentialis. The reading of that excellent treatise gave me occasion to consider the subject again. As my design related chiefly to the construction of tables, so I found and examined a great variety of methods which might be employed for that purpose: but at last I pitched upon one, which I judged to be by much the simplest and most convenient of any of those, which offered themselves to my thoughts. The account of this method may make a second part to be annexed to the paper I formerly sent you, if you and your mathematical friends shall think it of any use to be published. I have not as yet drawn it up in writing, otherwise it might have accompanied this letter. The best description I can give you of it in few words, is this; that in some respects it resembles Sir Isaac's method in Prop. iii. of his Meth. Diff. but is in the main more nearly related to the method of Mr. Briggs, described in cap. 13. of his Arithmetica Logarithmica, edit. Lond. Dr. Gregory, Lib. v. Prop. xxv. of his Astronomy, refers his reader to a book of Gabriel Mouton, De Observationibus Diametrorum Solis et Lunæ apparentium. Though I do not much rely upon the Doctor's recommendation, yet

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I should be glad to see the book, or to have some short account of his way. If you could assist me in this respect, I should be much obliged to you for the favour. I have formerly sent to my bookseller at London for it, but he could not procure it. I am very desirous to have the edition of the Principia finished; but I never think the time lost, when we stay for Sir Isaac's further corrections and improvements of so very valuable a book, especially when this seems to be the last time he will concern himself with it. I am sensible his other business allows him but little time for these things, and therefore I cannot hasten him so much as I might otherwise do. I am very well satisfied to wait till he has leisure.

I am, Sir, your hearty friend and servant,

ROGER COTES.

This letter is printed in the Gen. Dict. vol. iv. p. 444. It is an answer to one from Cotes to Jones, which is published by Lord Teignmouth in his Life of Sir William Jones, p. 8.

XCII.

COTES TO JONES.

Dear sir, Cambridge, Nov. 11, 1711.

I have received Mouton's book. I thank you for the favour you did me in sending it. I have looked over what relates to his way of interpolation, but I find no cause from thence to make any alteration. I beg your pardon that I have not yet sent you my second paper. I had finished it before I received your last letter with the book, but since that time I have not had leisure to transcribe it. I design to send with it a paper concerning logarithms, which together with the others may serve to fill up a whole Transaction, if

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you shall think them of any use to be published after you have read them. The controversy concerning Sir Isaac's philosophy is a piece of news that I had not heard of, unless Muys's Elementa Physices be meant. I think that philosophy needs no defence, especially when 'tis attacked by Cartesians. One Mr. Green, a fellow of Clare Hall in our University, seems to have nearly the same design with those German and French objectors, whom you mention. His book is now in our press, and is almost finished. I am told he will add an appendix, in which he undertakes also to square the circle. Ex pede Herculem; I need not recommend his performance any further to you. I am, Sir,

your obliged friend,
and humble servant,
ROGER COTES.

This letter is printed in the Gen. Dict. vol. iv. p. 444. An imperfect copy of it is likewise printed by Lord Teignmouth at p. 10. At p. 9 he has given Jones's letter of the 25th of October, to which this is an answer.

XCIII.

COTES TO JONES.

Dear sir, Cambridge, Nov. 25, 1711.

I thank you for M. de Moivre's Treatise of Chance. I have not yet had leisure to go over it. Mr. Saunderson, by whom you sent it, was on Tuesday last elected our Mathematical professor in the room of Mr. Whiston. I am not perfectly acquainted with him: as far as I can judge of him, he seems to have an extraordinary good genius. The want of his sight

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is certainly an insuperable disadvantage to him in several respects; but I believe in some others he has an advantage from it. This letter comes to return you my hearty thanks for the kind offer of your assistance in publishing my papers, and to acquaint you, that having lately had some discourse with a friend, I have by him been persuaded to alter my mind as to the printing them in the Transactions. Instead thereof I have now some thoughts of adding one or two things more, and printing them at our University press. I shall thereby have the advantage of a better letter and paper, and may correct the sheets myself, which trouble I must otherwise have begged of you to have undertaken, though it were unreasonable in me to do so.

I am, Sir, your
much obliged friend,
and humble servant,
ROGER COTES.

XCIV.

COTES TO JONES.

Sir, Cambridge, Feb. 13, 1713.

I have received your obliging letter, together with the very agreeable gift of the Commercium Epistolicum. I have delivered one copy to the University Library-keeper, another to the library-keeper of our college, and the third to Mr. Saunderson, as from the Royal Society. You may be pleased to return our acknowledgments of the favour. I am very glad to see this piece at length made public, in which quicquam cuiquam detractum non reperio, sed potius passim suum cuique tributum.

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Two or three years ago Mr. Keill proposed something, in a letter to Mr. Whiston, concerning the Keplerian problem, as either newly invented by himself or communicated by Dr. Halley. If he proceeds now upon the same foundation, and has not himself happened to take notice of the thing, you may acquaint him, that the approximation, sent in that letter to Mr. Whiston, may be seen in Ricciolus's Almagest, who ascribes the invention of it to Cavalerius. Hugenius also makes use of it in his Planetary Clock. I mention this out of respect to Mr. Keill, that the editors of the Acta Lipsica may not have any advantage over him, since we find they are so exceedingly officious in rendering to every one his own.

I am glad to understand that the problem of the refraction of the atmosphere is on foot.

I have sent Mouton's book by a friend who went from hence yesterday: he will leave it for you at Child's. I return you my thanks for the use of it. I am sorry I have given you any trouble about it, since I now find Dr. Bentley has it.

My own papers are not yet begun to be printed, and I almost wish I had not made you expect them. I have never yet printed any thing, and you must forgive me if I be somewhat timorous the first time. I believe I formerly told you that I thought to join one or two other papers, of my own, to those concerning logarithms and the differential method; I have since altered my design, and rather choose to print the last mentioned alone. If I find they are not ill received, I may venture hereafter to publish some others. But because I guess they alone may make but about eight or ten sheets, I am thinking to add to them two or three pieces relating to the same subject: as Mercator's Logarithmotechnia, the thirteenth chapter of

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Brigg's Arith. Logar., and so much of Mouton as relates to the differential method. But of these, or what else you shall think more proper, I shall be glad to have your opinion. As to your question concerning solstitial meridian altitudes, I cannot pretend to give you my advice, not having seen all the latest observations. The account of the meridian line in the church of St. Petronio in Bologna may possibly be something to your purpose. But this is only a guess, for I have not seen that book. I am, Sir,

your affectionate friend,

and humble servant,

ROGER COTES.

This is an answer to a letter from Jones of the 6th of Feb., which is printed by lord Teignmouth in his Life of Sir William Jones, p. 11.

XCV.

SAUNDERSON TO JONES.

Sir, Christ College, March 16.

The reason of my troubling you with this is, to desire you would favour me with your opinion of a late French piece, put out, as I am told, by M. Reyneau, and entitled, the Method of resolving Mathematical Problems. I am informed that the conic sections are there treated of in a much better method than in any other piece hitherto extant, though the title doth not seem to promise much of that kind. Mr. Cotes joins with me in desiring this favour of you, because he would willingly see how far that author's method may differ from or agree with a particular method, he has lately fallen into, of considering the conic sections,

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which is a very general one, as taking in also curves of the higher orders. There has been nothing published here since my last to you, excepting a treatise, which is not worth mentioning, by one Mr. Green, fellow of Clare Hall of this university. If there had been any thing in it instructive or diverting I should have sent it you; but I can find nothing in it but ill manners and elaborate nonsense from one end to the other. The gentleman has been reputed mad for these two years last past, but never gave the world such ample testimony of it before.

They are now got to the fourth proposition of the third book of Sir Is. Newton's Principia; but I cannot give you so full an account of the conduct of that piece, as perhaps you will desire, or as I should have done, but that I know Mr. Cotes maintains a correspondence with you, and I doubt not but he will give you an account of it, so far as he thinks Sir Is. will be willing to have any body acquainted with it. I have partly persuaded Mr. Cotes to communicate to you some thoughts of his concerning logarithms and their use in the mensuration of several curves, areas, surfaces, and solids; wherewith, I can promise you beforehand, you will be wonderfully entertained. All the parts of this little treatise do so entirely depend on the three or four first propositions, that 'tis impossible to give you an account even of the constructions to their uses without them, excepting that for the determining the length of the logarithmic curve, which, if I am not mistaken, Mr. Cotes told me he had already communicated to you. The constructions are most of them in a general scholium towards the latter end, but very few of them demonstrated. I have traced him in several of those he has not demonstrated,

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and have demonstrated them from his own principles[c], as indeed it is impossible to fall into those constructions upon any others. These demonstrations I shall not fail to communicate to you when you have perused the whole, if you desire it. These constructions you will find very simple, and of prodigious use in all manner of practice.

Pray, Sir, let me hear from you the next post, and you will very much oblige your very humble servant,

N. SAUNDERSON.

Sir Is. Newton is much more intent upon his Principia than formerly, and writes almost every post about it, so that we are in great hopes to have it out in a very little time.

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*XCV.

SAUNDERSON TO JONES.

Sir, Christ Coll. Feb. 4, 1713–14.

We have here proposals for a posthumous piece of Dr. Gregory, entitled, Notes upon Sir Isaac Newton's Principia, &c. I have made what inquiry I can here about it, but can meet with nobody that can give me any account of it. I hear you are concerned in the edition, and therefore should be very glad if you would favour me with your thoughts of the piece, and whether 'tis an explanation of the whole or only of some parts of the Principia; I shall be glad too to know what assistance Dr. Gregory has had, because it may be questioned whether Dr. Gregory (though no inconsiderable mathematician) was equal to a work of this kind. I am sorry I have had so much business

[c] See Saunderson's Fluxions, p. 129.

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upon my hands, ever since I had the happiness to be acquainted with you, as has hindered me from carrying on a correspondence with you; than which nothing to me can be more desirable; but I am in great hopes in a short time to be more at leisure. I have a lawsuit depending, which I believe will call me to town in about a month's time, where I hope to see you; in the mean time believe me,

your most humble servant,

NIC. SAUNDERSON.

Pray give my service to Mr. De Moivre, Mr. Machin, Mr. Keill, &c.

These notes on the Principia were not published, and most probably are the same, which are mentioned by Dr. Gregory in the Transactions of the Royal Society of Edinburgh, vol. xii. p. 71.

XCVI.

REYNEAU TO JONES.

Monsieur,

Je ne saurois assez vous marquer ma reconnoissance du recueil des excellentes pièces de mathématiques de Monsieur Newton, que vous m'avez fait l'honneur de m'envoyer. J'ai une ardeur extrême de voir tout ce qui vient de ce grand homme, à qui on doit tant d'utiles découvertes, persuadé que j'y trouverai toujours de quoi profiter. Tous ceux qui aiment les mathématiques vous savent bon gré, monsieur, du soin que vous avez puis de recueillir tant de bons morceaux de cet illustre original, qui seroient perdus pour eux, et de la facilité que vous leur donnez de les entendre par la belle impression, par les titres qui distinguent les différentes matières, et en mettant les figures dans tous les lieux qui leur conviennent. J'ai vu avec

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plaisir dans ce recueil les premières découvertes de cet excellent auteur, qui seroient des chef-d'œuvres pour les autres, et comment il les a portées à leur entière perfection. J'avois vu, dans la première édition du savant et profond ouvrage des Principes de la Philosophie Naturelle, les belles applications qu'il fait de ses méthodes à découvrir tout ce qu'il y a de plus caché dans la nature. La seconde édition en est encore si rare ici, que je n'ai pu la voir que des instants par le moyen de ceux, qui ont enlevé ce qui en étoit venu d'abord; mais j'y ai vu, avec plaisir, que ce que l'on avoit repris dans quelques journaux de Leipsic, qu'on m'avoit montrés, sur la résistance des milieux, n'étoit que des erreurs de calcul, comme je le jugeai en les lisant, parceque je savois que sa méthode de prendre les secondes différences, les troisièmes etc. n'étoit pas celle qu'on lui attribuoit, mais celle qu'il a un peu plus expliquée dans l'endroit de la nouvelle édition, où il rétablit ce qui n'étoit qu'erreur de calcul dans la première.

J'espère, Monsieur, que vous aurez la bonté de m'excuser d'avoir différé si long-tems de vous remercier. C'est le défaut d'occasions d'envoyer surement des lettres en Angleterre, qui en est la seule cause. On m'en avoit promis par les quelles je me donnois l'honneur de vous écrire, qui ont toutes manqué. J'ai vu toute apparence que celle qui se présente, seroit plus sure, et j'en profite, avec plaisir, pour vous assurer qu'on ne peut être avec plus de respect et de reconnaissance que je suis,

Monsieur,
votre très humble et
très-obéissant serviteur,
REYNEAU,
de la congrégation de l'Oratoire.

à Paris le
23e Novembre, 1714.

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Jones published the Analysis per quantitatum series, &c. in the beautiful manner so justly described in this letter, in 1711. He seems to have been pleased with the compliments paid him by Reyneau, for there is, among his papers, a translation which he made of this letter into English.

XCVII.

MACHIN TO ———.

Dear friend, Jan. 25, 1715, Gresham College.

I have ventured to send you a solution of M. Leibnitz's problem, which you were giving me an account of yesterday in the afternoon; but because I have not seen the proposition in his own words, perhaps my solution may not be put exactly in that form as it ought to be; for which reason I would not have you shew this to any one, except Dr. Halley. When I have got a copy of the proposition itself, I'll go over it again, and endeavour to polish the demonstration, which is here omitted.

I am your assured friend,
and very humble servant,
J. Machin.

P.S. I believe you'll easily perceive how the construction I use may be made, with some little alteration, to serve when the given point of the curve is any where out of the axis of the hyperbola, as also how the like construction may be made for ellipses; but more of this when I see M. Leibnitz's own proposition.

Yours, J. M.

The problem here alluded to is that which Leibnitz sent to the Abbé Conti, and which Newton solved so immedi-

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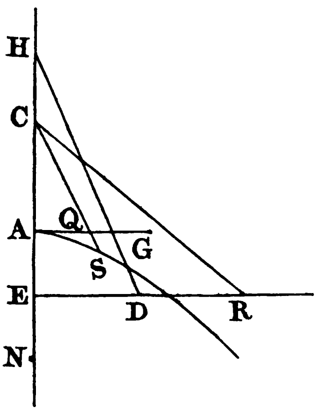
ately. Machin's letter was most probably addressed to Jones, and was certainly communicated to Halley. There are some memoranda (unconnected with the subject) in his hand on the outside; but what is more conclusive, he communicated the solution, in a corrected form, to the Royal Society, on the 2nd of February, 1715.

PROBLEMA LEIBNITZII.

Datis centro, latere transverso, verticeque principali cujusvis hyperbolæ, curvam quandam ducere per datum in axe punctum, quæ omnes hyperbolas, ex eodem centro, iisdemque verticibus principalibus, descriptas ductu suo orthogonaliter secabit.

Designet C centrum, A verticem principalem, AC dimidium lateris transversi hyperbolæ æquilateræ AD[a], N punctum in axe CAN datum, per quod curva ND talis duci debet, quæ omnium hyperbolarum, centro C vertice principali A descriptarum, curvis perpendiculariter occurrat.



A vertice A erigatur AG ad axem AC normalis et æqualis; sumtoque in axe puncto quolibet E erigatur perpendiculum infinitum EDR, axi AC adjiciatur recta CH ipsi EN æqualis; secatur recta AG in Q, ita ut AG + AQ sit ad QG, ut CN, distantia inter centrum C et punctum datum N, ad CE distantiam inter idem centrum et punctum assumtum E; jungatur CQ et producatur ad hyperbolam in S, ita ut abscindat aream hyperbolicam CAS.

[a] Sic in MS.—The curved line through ND has been inadvertently omitted in the figure.

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In perpendiculo infinito EDR abscindatur recta ER, talis ut quadratum, quod fit ex ipsius dimidio, areæ hyperbolicæ CAS equabitur; jam si centro H radio HD, distantiæ CR æquali, describatur circulus occurrens perpendiculo infinito ER alicubi in D, punctum concursus D in curva locabitur, quæ omnes hyperbolas centro C, semilatere transverso CA, verticeque A descriptas, perpendiculariter intersecabit. Eademque ratione puncta numero infinita determinari licet et propterea curva determinabitur. Q. E. D.

XCVIII.

MACHIN TO HALLEY.

Sir, Feb. 2, 1715.

After I considered the construction which Dr. Taylor[a] gave for Leibnitz's problem, I found it so exceeding neat and simple, that I was quite out of love with my own; but however, because I promised to send you a correction of an erratum which was in the copy you saw, I have taken the pains to draw it up in another form, which I think is better, and withal have added a rule for finding the curvity, which I believe will furnish an easier method of describing it, than that of finding a great number of points.

I am, Sir, with all respect,
your most obliged,
humble servant,
J. Machin.

[a] See Phil. Trans. vol. xxx. p. 695.

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XCIX.

COTES TO JONES.

Dear sir, Cambridge, May 5, 1716.

I received your letter from Mr. Parker's hands. I shall be very glad if I can any way serve that young gentleman. I take it very kindly of Dr. Taylor that he has been pleased to give so favourable a representation of my tables; and your letter encourages me to give you some account of the principal part of that work, with which I did not acquaint the Dr. by reason of his short stay with me. I must therefore tell you that geometers have not yet promoted the inverse method of fluxion by conic areas, or by measures of ratios and angles, so far as it is capable of being promoted by those methods. There is an infinite field still reserved, which it has been my fortune to find an entrance into. Not to keep you any longer in suspense, I have found out a general and beautiful method, by measures of angles and ratios, for the fluent of any quantity which can come under this form $\frac{d\dot{z}z^{\theta\eta+\frac{\delta}{\lambda}\eta-1}}{e+fz^{\eta}}$, in which d, e, f, are any constant quantities, z the variable, η any index, θ any whole number affirmative or negative, $\frac{\delta}{\lambda}$ any fraction whatever. The fluents of this form, which have hitherto been considered, are $\frac{d\dot{z}z^{\theta\eta-1}}{e+fz^{\eta}}$, $\frac{d\dot{z}z^{\theta\eta+\frac{1}{2}\eta-1}}{e+fz^{\eta}}$; these, you remember, are Sir Isaac's two first; and from these all his others are easily deduced. And as his irrational forms of the quadratic kind are derived from the rational, so from my general rational form I deduce irrational ones of all kinds.

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Thus, for instance, if $\frac{\delta}{\lambda}$ represent any affirmative or negative fraction, the fluent of any quantity of this form $d\dot{z}z^{\theta\eta-1} \times \overline{e+fz^{\eta}}|^{\frac{\delta}{\lambda}}$, or of this $d\dot{z}z^{\theta\eta-1}\overline{\frac{e+fz^{\eta}}{g+hz^{\eta}}}\Big|^{\frac{\delta}{\lambda}}$, and so of some others, depends upon the measures of ratios and angles.

M. Leibnitz, in the Leipsic Acts of 1702, p. 218 and 219, has very rashly undertaken to demonstrate that the fluent of $\frac{\dot{x}}{x^4+a^4}$ cannot be expressed by measures of ratios and angles; and he swaggers upon the occasion (according to his usual vanity), as having by this demonstration determined a question of the greatest moment. Then he goes on thus: As the fluent of $\frac{\dot{x}}{x+a}$ depends upon the measure of a ratio, and the fluent of $\frac{\dot{x}}{xx+aa}$ upon the measure of an angle, so he has more than once expressed his wishes that the progression may be continued, and it be determined to what problem the fluents of $\frac{\dot{x}}{x^4+a^4}$, $\frac{\dot{x}}{x^8+a^8}$, &c. may be referred. His desire is answered in my general solution, which contains an infinite number of such progressions. I can go yet further, and shew him how to find by measures of ratios and angles, without any exception or limitation, the fluent of this general quantity $\frac{d\dot{z}z^{\theta\eta+\frac{\delta}{\lambda}\eta-1}}{e+fz^{\eta}+gz^{2\eta}}$, or even this, $\frac{d\dot{z}z^{\theta\eta+\frac{\delta}{\lambda}\eta-1}}{e+fz^{\eta}+gz^{2\eta}+hz^{3\eta}}$, where θ, as before, represents any integer, and the denominator λ of the fraction $\frac{\delta}{\lambda}$ represents any number

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in this series 2, 4, 8, 16, 32, &c. any whole number being denoted by its numerator δ. In truth I am inclined to believe that Leibnitz's grand question ought to be determined the contrary way, and that it will be found, at last, that the fluent of any rational fluxion whatever does depend upon measures of ratios and angles, excepting those which may be had in finite terms, even without introducing measures.

I must now beg your assistance and management in an affair, which I cannot so properly undertake myself, especially by letters. You know Sir Isaac has left his sixth form imperfect, and under a limitation. This, though it does not lessen my opinion of the author, yet it appears as an eyesore to me in so beautiful a work. The very great respect and honour, which is due to him upon all accounts, makes me wish it were removed by himself. Pray therefore let him know that I can take off that limitation, and make this form as perfect as the others. And use all the address you have to make him set upon the same thing, and let me know that he has done it. My design is to mention it in my treatise; that he hearing from you what I had done, did himself, at your request, reconsider his sixth form, and very easily made it perfect.

I here send you a transcript of what I have set down for my memory about it. You see I happened to alter the appearance of the form a little, putting $\frac{d\dot{z}z^{\theta\eta-1}}{e+fz^{2\eta}+gz^{4\eta}}$ instead of $\frac{d\dot{z}z^{\theta\eta+\frac{1}{2}\eta-1}}{e+fz^{\eta}+gz^{2\eta}}$, but this makes no real difference as to the matter in hand. There are three ways (and I think there can be no more) of resolving the fluxion proposed into fluxions of other forms. The first way resolves it into two fluxions of

Sir Isaac's second form; this way is coincident with that which he has taken. The other two ways do each resolve it into four fluxions of Sir Isaac's fifth form. By collecting the several parts, and substituting the values of p, q, r, s, t, you will very easily be satisfied of the truth of these rules.

Positis $p = \frac{1}{2}f + \sqrt{\frac{1}{4}ff - eg} \qquad q = \frac{1}{2}f - \sqrt{\frac{1}{4}ff - eg}$

$$r = \sqrt{eg}$$

$$s = \sqrt{-fg + 2\sqrt{eg^3}} \qquad t = \sqrt{-fg - 2\sqrt{eg^3}}$$

$$\text{I. Erit } \frac{d\dot{z}z^{\theta\eta - 1}}{e + fz^{2\eta} + gz^{4\eta}} = \frac{g}{q - p} \times \frac{d\dot{z}z^{\theta\eta - 1}}{p + gz^{2\eta}} + \frac{g}{p - q} \times \frac{d\dot{z}z^{\theta\eta - 1}}{q + gz^{2\eta}}$$

$$\text{II. Vel} = \frac{g}{2r} \times \frac{d\dot{z}z^{\theta\eta - 1}}{r + sz^{\eta} + gz^{2\eta}} + \frac{gg}{2rs} \times \frac{d\dot{z}z^{\theta\eta + \eta - 1}}{r + sz^{\eta} + gz^{2\eta}} + \frac{g}{2r} \times$$
$$\frac{d\dot{z}z^{\theta\eta - 1}}{r - sz^{\eta} + gz^{2\eta}} - \frac{gg}{2rs} \times \frac{d\dot{z}z^{\theta\eta + \eta - 1}}{r - sz^{\eta} + gz^{2\eta}};$$

$$\text{III. Vel} = \frac{g}{2r} \times \frac{d\dot{z}z^{\theta\eta - 1}}{r + tz^{\eta} - gz^{2\eta}} - \frac{gg}{2rt} \times \frac{d\dot{z}z^{\theta\eta + \eta - 1}}{r + tz^{\eta} - gz^{2\eta}} + \frac{g}{2r} \times$$
$$\frac{d\dot{z}z^{\theta\eta - 1}}{r - tz^{\eta} - gz^{2\eta}} + \frac{gg}{2rt} \times \frac{d\dot{z}z^{\theta\eta + \eta - 1}}{r - tz^{\eta} - gz^{2\eta}}.$$

Si e, g, sunt signis dissimilibus, solvitur per I.

Si e, g, sunt signis similibus, et f dissimili, solvitur per II.

Si e, f, g, sunt signis similibus, et $ff > 4\,eg$, solvitur per I.

Si e, f, g, sunt signis similibus, et $ff < 4\,eg$, solvitur per II.

Si e, g, sunt signis similibus, f dissimili, et $ff > 4\,eg$, solvitur per I, II, III.

Your's, &c.

R. COTES.

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Be pleased to communicate the contents of this letter to Sir Is. only. I have some thoughts of touching Mr. L., and would not have his agents inform him upon what points; which might produce something to hinder my design.

C.

MACHIN TO HALLEY.

Sir, July 16, 1716.

The equation I gave you last night, for the little triangle, was just double what it ought to be, as you will easily see, if you please to cast your eye on the rule in my paper.

I find that Sir Isaac's rule, as it is in the last edition, will do for the orb of Mars very exact, and that almost as well at 120 degrees, mean anomaly, as at 60. The reason of which is, the first equation y is near upon as much less than Bullialdus his equation, as the little triangle amounts to. For the greatest equation of Bullialdus in the orb of Mars, according to your eccentricity, is $7' \; 27\frac{2}{3}''$; and so it comes out by Sir Isaac's rule in his first edition, but in the last it comes only to $7' \; 23\frac{7}{10}''$.

After I came home last night, I computed the several equations for 60 and 120 degrees by my way, which I take leave to trouble you with. The greatest segment is $1' \; 50'' \; 8$. The greatest triangle is $0' \; 5''$.

		°	′	″
The mean anomaly . . .		60	0	0
Bullialdus's equation	+	0	6	27
The segment.	+	0	1	12
The triangle	−	0	0	5
The angle at the focus		60	7	34

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	°	′	″
The mean motion	120	0	0
Bullialdus's equation	− 0	6	27
Segment	+ 0	1	12
Triangle	+ 0	0	5
The angle at the focus	119	54	50

I am, Sir,

your most obliged humble servant,

J. MACHIN.

CI.

MACHIN TO HALLEY.

Dear sir,

Since you have been so kind as to offer me your assistance, in furnishing me with a few places of the moon observed, I take the liberty to put you in mind of your said promise. You said you had one month's observations which contained near twenty days: if I could have those places, as determined from your observations, it would be of great service to me. Or, if that cannot be done, without too much trouble to yourself, it will be of great use, if I could have a few places of the moon when in the octants or near, and when the small equations, called the first semestrial and the fourth, happen near their maxima, having the same sign with the variation. If I could have possibly come out myself, I would not have failed to have waited upon you for this favour, but hope you will send any such as are ready at hand, and are under the character before described, by the hands of Mr.

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Motte, who is to be at Greenwich; wherein you will very much oblige, Sir,

your devoted servant,
at command,
JOHN MACHIN.

P.S.

I could wish to have as many as could be in days following one another, before and after the octants; but beggars must not be choosers. I shall, upon all occasions, be ready to acknowledge gratefully any service, which you are pleased to do me herein. Mr. Motte stays for this, therefore I hope you'll excuse the haste of yours, J. M.

The address of this letter is lost; but there can be no doubt of its being written to Halley, some of whose figures are to be seen written on the blank part of the paper. Many years of Machin's life were employed in the examination of the moon's motions, and endeavours to improve the Tables of them. The Royal Astronomical Society is in possession of a large mass of his papers, which prove the zeal and industry with which he pursued his object.

There being no date to this letter, the exact time when it was written is very uncertain.

CII.

REYNEAU TO JONES.

Monsieur,

Je suis pénétré de la plus vive reconnoissance des marques, que vous me faites l'honneur de me donner de votre souvenir, par les beaux endroits de vos Transactions, que j'ai reçus de votre part. Je profite du départ pour Londres de celui de Messieurs vos con-

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frères de la Société Royale, qui m'a apporté le dernier morceau, pour vous la marquer. J'ai lu, ou plutôt je me suis fait expliquer les matières, contenues en Anglois, dans les pièces que vous m'avez envoyées. J'étois au fait de l'extrait du Commercium Epistolicum, ayant lu le Commercium même, qui est en Latin : et j'avois lu auparavant, avec bien du plaisir, le premier Traité d'Analyse par les Equations infinies, et tout l'excellent recueil des pièces de M. Newton que vous m'aviez fait l'honneur de me donner. Je n'avois pas besoin de cela pour m'assurer que M. Newton avoit trouvé le calcul différentiel et intégral, comme nous les nommons ici, dès avant le tems qu'il écrivoit les deux lettres à M. Leibnitz, en 1676, qui sont dans le troisième volume de M. Wallis. Ces deux lettres, qui contiennent de si beaux usages de ces méthodes, et qui m'ont été à moi-même si utiles pour apprendre ces nouveaux calculs, me l'avoient assez fait juger. La résolution[b] du problême proposé autrefois par M. Bernoulli, qui est dans le cahier des Transactions, que je viens de recevoir de votre part, est bien succincte; mais elle suffit pour faire voir aux connoisseurs le moyen d'arriver à l'équation, et le problême paroîssant de peu d'usage, comme on le marque à la fin de la résolution, et n'étant qu'un exercice d'esprit, il étoit inutile de prendre la peine d'en donner une résolution complète et entièrement détaillée.

Je suis bien fâché, Monsieur, de n'avoir, de mon côté, rien à vous communiquer, qui pût vous faire plaisir, et vous paroître nouveau; mais je ne suis occupé depuis long-tems qu'à travailler pour les commençans et à leur faciliter vos belles découvertes—celles de l'illustre et du savant M. Newton, et de Messieurs vos au-

[b] Probably Newton's. See Phil. Trans. vol. xxix. p. 399.

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tres confrères, académiciens d'Angleterre. Je suis, Monsieur, avec tout le respect et toute la reconnoissance possibles,

votre très-humble et
très-obéissant serviteur,
REYNEAU, de l'Oratoire.

à Paris ce 29e Août,
1716.

CIII.

BROOK TAYLOR TO JONES.

Sir,

Though for reasons, that you too well know, it was impossible for me to desire that Sir Isaac Newton should see my paper[d] against Bernoulli before it was published; yet I am not displeased at what you have done; because I am sure you would not have shewed it him, if you had not been sure it could be no inconvenience to me; and in what he says of it I find myself a gainer, for the place that piece has in the Philosophical Transactions, will shew the world that what I say has the approbation of the Royal Society, which is an advantage that I have no manner of reason to despise, though I never intended to ask it, being much more desirous that my actions should subsist and be supported by their own reasonableness, than by the authority of any other persons whatsoever. I am very sensible that Sir Isaac Newton is very glad that any thing should be published, which affects Bernoulli, and believe that is the only reason, which makes him willing this piece should appear in the Transactions. But before I determine to let it appear, I have many

[d] Phil. Trans. vol. xxx. p. 695

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other things to consider, wherein I particularly desire your opinion and advice. 1. Whether I really have sufficient provocation thus to attack Bernoulli, as my passions make me believe I have; 2. whether my reputation would suffer or no, if I should let it alone; 3. whether I am not mistaken in any things I say; 4. whether I say too much, or enough; 5. whether my expressions are clear, and my language as it should be; 6. lastly, whether I have said any thing that is not agreeable to the character of one, who is truly sensible of the duties of honour and good manners. In these things I must claim your serious and free opinion; for as I am fully determined, (as I say in the conclusion,) never any more to meddle in this wrangling way, it concerns me very much that what I say should not be liable to blame. What reason is there that Sir Isaac Newton should keep the paper? I had rather you had it, that I might have the benefit of your opinion.

I am very much obliged to you for the printed paper you sent me, which I think appears very fair; but I am sorry that work goes on so slow, and think it a very great shame. If you have not yet seen the Exercitationes subsecivæ Francofurtenses, they are worth your looking into, upon account of some things of Herman; I mean the poor man's mistakes. He shews a prodigious respect for his master Leibnitz, and takes no small pains to defend his whimsical notion, of the absolute force in bodies being as the perpendicular heights from whence they fall, even at the expense of most apparent self-contradiction. Another of his mistakes I am concerned in. It is in his discourse on the vibrations of a string. I believe he will take the first occasion to correct those errors himself; I mean the last. In one paper he very well shews that

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the cause of gravity is not yet found out, though he seems to think it not entirely to be despaired of. He vindicates himself, I think, very handsomely against Dr. Keill, who had charged him with having made use of an invention of his without naming him, and shews a disposition to treat every body with civility and justice. He gives greater testimonies to Sir Isaac Newton than I have seen from any of the foreigners; though in one place he seems to wish that Leibnitz might have a share in his discovery of universal gravitation. I would transcribe the passage, if I had not lent out the book. By my correspondence with M. Montmort I find Herman is not of Bernoulli's party, but is rather under his displeasure.

Pray, do what you can to make Mr. Smith go on with Mr. Cotes's work. What says Sir Isaac Newton to Maclaurin's paper about the description of curves? It seems to be very curious. That gentleman certainly deserves to be encouraged.

I am,
Dear Sir,
your most obliged,
humble servant,
BROOK TAYLOR.

Bifrons,
5 May, 1719.

CIV.

MACHIN TO JONES.

This letter, written (Aug. 4, 1727) neither with judgment nor good taste, blames Sir Isaac Newton's treatment of the moon's motions, and ends with an assurance of his having himself completely resolved the problem, so as to entitle him to the parliamentary reward of 10,000*l*. There is a rough copy of Jones's answer, in which he congratulates him on the prospect of success, but cautions him not to be sanguine in the expectation of completely realizing it.

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CV.

MAUPERTUIS TO JONES.

Parisiis, 1a Sept. 1729.

Humanissimam, vir illustrissime, epistolam tuam accepi et munera; urbanitati tuæ et modo quo das nil addi poterat nisi pretium muneris: et quanquam propter Anglicum sermonem mihi prorsus ignotum, et rerum arduitatem ingenii mei vires et scientiam nimium superantem, non potui quin obstupescerem, videns magni Newtoni miraculis etiam nunc nova superaddi miracula. Vestrum est viri immortalis scientiam perfecisse novam, et in vestra Britannia natam physicam cœlestem. Me vero jamdudum ab omni studio optimæ et carissimæ matrum fata revocant, et omni mœrore et ærumna afflictum tenent.

Ad nostros, quos epistola tua jubebat, misi dona; excepto P. Reyneau, (qui de medio excessit,) et omnes gratias tibi maximas agunt; mihi autem, vir illustrissime, nil tam exoptatum est, quam ut tibi bene tester quantum amicitia, qua me afficere dignatus es, glorior, et quantum eam pretiosissimam mereri desiderem.

Nuperis diebus celeberrimus vester D. Halley inter nostros, quos Academia habet socios extraneos Bianchini loco annumeratus est. Superest ut tibi offeram hunc unum, quem novum in re mathematica habeamus libellum, et alia duo exemplaria ad clarissimos viros D. de Moivre et Desaguliers, cum salutatione nostra, mittas quæso. Vale, vir clarissime, mihi bene velle pergas, et tibi omni cultu neminem me credas devinctiorem.

MAUPERTUIS.

Tuam epistolam, 14 Junii scriptam, paucis tamen abhinc diebus accepi.

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CVI.

LOGAN TO JONES.

My kind friend, Philadelphia, 8th of Nov. 1732.

I must with gratitude acknowledge thy obliging present of three little books, by thy quondam pupil J. Georges, though a line from thy hand would have rendered them still more acceptable. I could not, on sight of them, but very much admire at those great improvements by J. Machin [e] on Sir Isaac's theory of the moon, now published so soon after that great man's death, and not mentioned before, which, had they been early enough communicated to so industrious a hand, might perhaps have been formed into that method and order, fortified with proper demonstrations, which they at present seem to want. I am willing to impute it to my own incapacity, that I can by no means comprehend his law of motion, where a body is deflected by two forces tending constantly to two fixed points, viz. that it will describe, by lines drawn from the two fixed points, equal solids, in equal times, about the line joining the said fixed points, which are his words, but such as I can, by no construction of them, form a notion of a solid that will duly increase in quantity in any wise proportionally to the times of description: nor can J. Georges, though he appeared at first acquainted with the thing, give me any light into it. A young man here, also, of an excellent natural genius for these studies, who, under the greatest disadvantages of education and circumstances, has made himself a very good master of the Newtonian philosophy, and that great author's writings, finds himself as much at a loss in it as I do: and yet perhaps one line might render

[e] Probably, The Laws of the Moon's Motion according to Gravity, published with Motte's translation of the Principia in 1729.

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it intelligible to us. But I cannot pretend a right to desire such a favour, however obliging it might prove.

That young man, having not long since invented an easy and curious instrument for taking at sea the moon's real place in the heavens, by her distance from any known fixed star near the ecliptic, though 50 or 60 degrees distant from the moon, and this very near, if not altogether, as exactly as it could be done by a real transit, which is performed by means of one small speculum, with a much smaller visual passage through it, fixed a few inches before the further end of a short telescope of 30 or 36 inches, and another such small speculum but wholly reflective, fixed at the end of a label or index, of about the same length with the telescope, moving on a centre exactly under the reflecting surface of the speculum erected on it, and by its fiducial edge below marking the graduations, accounting one degree two, on a short limb at the lower end of the piece, on which the telescope is fixed; that young man, I say, having invented this, and shewn me one he had made, believing it might prove of great use for the purpose above mentioned, in May or June last I sent Dr. Halley an account of it, with a figure and description of the instrument to be applied by him as he should think proper. How it may appear to his better judgment, I cannot guess; but if thou please to make some little inquiry into it, in a proper manner, and favour me with a line upon this letter, I shall be highly obliged to thee, which to save trouble may be directed to me, in a cover to Lawr. Williams, merchant, and left at the Pensilvania Coffee-house in Birchin Lane, from whence he will carefully forward it to

thy sincere and most obliged friend,

J. LOGAN.

10th Nov. P.S. Since the above, Tho. Godfrey, the

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young man I mentioned, has, I think, hit on the meaning of that rule of J. Machin; but, according to his account of it, 'twill be of no use when the body moves in the same plane with the centres; for then there can be no solid. I suppose he will take the freedom to write to thee on another subject. He wishes I had directed my letter, with his invention of the reflecting instrument, to thee, instead of Dr. Halley.

J. L.

This letter alludes to a curious circumstance, which long created a difference of opinion on the right to an important invention, which was claimed both by England and America. The following dates, however, set the question at rest. Hadley completed one of his reflecting quadrants in the summer of 1730, and in the following May (of 1731) he communicated an account of the instrument to the Royal Society. Tho. Godfrey contrived a similar instrument in October 1730, which he had immediately constructed for his use; and in 1732 Godfrey and his friend Mr. Logan both wrote to Dr. Halley to give him an account of what had been done at Philadelphia. Of this, more occurs in Logan's next letter; in this place it may be sufficient to say, that the inventions appear to have been clearly independent of each other, and that Hadley's had the priority in point of time.

CVII.

LOGAN TO JONES.

Esteemed Friend, Pensilvania, 12th of Nov. 1734.

About this time two years, by Capt. Wright, I returned thee my thanks for thy kind present of two or three small pieces delivered me by J. Georges. Since which I have been informed by my friend P. Collin-

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son[f] and said Wright, of thy generosity and justice in asserting before the Royal Society, the right of an inventor (at least), if not absolutely the first, of the reflecting instrument, to Tho. Godfrey, as well as in vindication of my reputation from the slur, that Dr. Halley's unhandsome conduct towards me had like to have thrown on it, in which he was highly ungrateful; since nothing but my respect for him could induce me to communicate to him, preferably to all others, what he might easily judge, from my letter, I thought would be wholly new to him. And to suspect a trick or sham in it, he must have considered me as one of the most senseless or maddest creatures upon earth, if I should voluntarily, in so wild a manner, expose myself even to a hazard of the vile imputation of an impostor in a matter, wherein I proposed to myself neither credit nor profit, nor any advantage whatsoever. Thy kind endeavours, therefore, to obtain justice both to T. G. and me deserve my hearty acknowledgments, which I here take the freedom to make thee; and I should do the same to the ingenious J. Machin, to whom I find I was also particularly obliged, but not having the honour of any acquaintance with him, I request thee, when you meet, to do it for me in my name.

Being told you had wished for a fuller account of T. Godfrey's improvement of the mariner's bow, than he had himself given of it, which he sent without my knowledge, I took the trouble of drawing up one, this last summer, and sent it to P. Collinson, which, if he has received and communicated it[g], I hope will give satisfaction, as I also do, that T. G.'s merit in

[f] For an account of Mr. Collinson, see Phil. Trans. Abr. vol. vii. p. 368.

[g] See Phil. Trans. vol. xxxviii. p. 441.

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the other instrument will not be forgotten; for as that instrument of his was not only made, but used at sea six months before J. Hadley's was seen or known, and my description of it will, I suppose, be allowed to be much plainer, and the use of it applied to nobler purposes in my letter, T. G. justly deserves the preference. To have sent a demonstration of the principle, on which the instrument is formed, to Dr. Halley, would have been very needless; but I drew one myself, which, if I can find it, and have time, I shall send thee, that if I mistake not, will appear somewhat clearer than J. H.'s. I thought also to have mentioned to thee some other mathematical subjects, which at present I shall defer, and conclude this with sincere respect from

thy much obliged friend,

J. LOGAN.

P. S. The ship staying longer than expected, I have not only sent the demonstration mentioned above, but another that I have now struck out, which I take to be preferable to all others, for the reasons given at the close of it.

J. L.

Logan's first method is the same as is now to be found in all books of optics, where it is demonstrated that the angle made with one another by the reflecting planes is half of that which will be contained by the directions of the incident and reflected ray. He then goes on to say as follows:

The preceding demonstration, I suppose, will be allowed strictly geometrical; but to those, who require not such, this other may appear more plain and satisfactory, as it not only sufficiently proves the proposition, but at the same time also shews the reason, and that it must be so.

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Let the glasses AB and CD (Plate 3, Fig. 1) be supposed to be exactly parallel, and the ray IE, falling on AB, make with it the angle IEA, which being reflected from E on the glass CD in the line EF, and again from F in Ff, makes the four angles IEA, BEF, CFE, and DFf, all equal, and Ff and IE are parallel, or the ray IE continued in the same direction, and let each of these angles be called $z = 1$.

Let the glass AB be moved from its parallel position into that of aEb, making with its first (now the line AB) the angle aEA $= \frac{1}{4} z =$ the angle bGD, which reduces $z = 1$ to $\frac{3}{4} z =$ the angle IEa, and the progress of the ray IE will now (by Dioptr.) be IE, EH, HK. And because the angle HEb = the angle IEa $= \frac{3}{4}$, because the angle bEB (part of it) $= \frac{1}{4}$; therefore the angle HEB $= \frac{2}{4}$ only. Let Hh be drawn parallel to IE, then because AB and CD are parallel, the angle DHh is [equal to] the angle IEA $= z = 1$, and for the same reason the angle BEH = the angle CHE = the angle DHK $= \frac{2}{4}$. But the angle DHh $(=1)$ − the angle DHK $(= \frac{2}{4})$ is $= \frac{2}{4}$ = the angle KIE = (because the angle bGD = the angle AEa $= \frac{1}{4}$) = twice the angle bGD. Q. E. D.

A bare view of the figure, and of the progress of the ray IE in the different positions of the glass AB, ab, will abundantly shew the reason of this; and therefore, though the first demonstration is more agreeable to the strict method of the ancients, yet this for instruction is preferable. T. Godfrey also shewed me his, which was a good one; but as it was neither altogether so geometrical as the first of these, nor so clear as the last, I have now taken the freedom to send these two of my own.

James Logan was born at Lurgan, in the county of Armagh, on the 20th of Oct. 1674. At thirteen he was bound appren-

tice to a linendraper in Dublin; but in consequence of the disturbances which occurred after the Revolution of 1688, he came over to his parents, who were then settled at Bristol. He was there engaged in the trade with Ireland till 1699, when he accompanied Wm. Penn, as secretary, to America. He was afterwards left there by Penn, and took an active part in the management of the colony. He appears to have been highly respected by his fellow-citizens, and died Oct. 31, 1751. He was a botanist, and maintained a correspondence with Linnæus: he was acquainted with modern as well as the learned languages, and had collected an extensive library, which he left for public use.

Mr. Logan's complaints were occasioned by Halley's not reading to the Royal Society the letter which he had received in 1732. As Hadley's account of his instrument had been printed in the Philosophical Transactions for 1731, there might have appeared to be no pressing occasion for a second communication on the same subject. Halley was seventy-six years of age at the time, and betrayed a remarkable instance of forgetfulness on this very subject. On Hadley's paper being read, he claimed the original invention of the reflecting quadrant for Newton; he referred to a wrong document, and then explicitly gave up the claim, although Newton's description must have been at the very time among his own papers. There is no need, therefore, for having recourse to any supposed imposture on the part of Mr. Logan. Jones brought the question before the Royal Society in Jan. 1734, and Godfrey's claims to be an inventor (though not the first) were established. It must be particularly noticed that Logan makes no accusation of Hadley having taken the idea of his instrument from Godfrey's invention; which is an accusation (though wholly unfounded) that has been commonly made by American writers.

CVIII.

LOGAN TO JONES.

My good friend, Pensilvania, 14th of June, 1736.

Though I have no absolute right to claim so much

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of thy regard, as to believe thee from thence under an obligation to answer my letters; yet I have had such proofs of it in other respects, that I really thought I might at least hope for the favour, and I still so far continue of the same opinion, that I here venture once more to solicit it on an occasion, that is of some importance to a subject I have now in hand: and my reason for troubling thee with it is, because I know none more capable of answering my doubts, or of whom, notwithstanding thy past silence, I can more freely request such a favour.

The case is this. My former busy life having never allowed me to look so deeply into the mathematics as otherwise I might, I entirely declined considering the doctrine of infinite series with fluxions till lately, when being desirous to find some certain method or rule for knowing the length of an elliptic curve, and meeting with no other than that of series by fluxions, I resolved to try this, and meeting with those two in Sir I. Newton's letter of the 13th June 1676, published in the Commercium, 8vo. edit. p. 137, 138, the first for finding one part by the absciss from the centre, and the other for that taken from the vertex, I doubted not but I was master, or soon should be so, of what I sought. But, vastly to my disappointment, I found in working on them, that the last, which should give the most important part of the curvature, is no converging series at all; but after a few steps in the quotient, unless x be taken very small, (and then it is of no use,) it actually diverges, if I may use the expression; I mean that the differences increase and grow wider.

Surprised at this, and being diffident of myself, I got T. Godfrey, who has more youth on his side, and an excellent natural genius for such studies, to try

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what he could make of it; but he found it just as I did. As this could not but raise my wonder, I applied myself more closely to consider the process, and there discovered the reason, viz. that the series from which this root (in p. 138.) is extracted, has its second, or some other near term, bigger than the first, (it is the unit or 1, in the second, third, or fourth term, which exceeds the first,) and therefore it must be impossible in nature ever to deduce any good series from it.

Being disappointed in this, I tried what might be done for the same part by throwing out x and working on y; and this succeeded greatly to my satisfaction; for I found another series, by which I could continue this last on y as far as I pleased, without extraction, (a point I laboured for in the other on x, but could never find it.) I also struck out a canon, by which I could with much greater expedition and certainty carry on the operation in numbers; and from these steps I doubted not but I was master, though with more labour than I expected, of all I had proposed by it; and yet in the close I found it all wholly lost, otherwise than that I had the satisfaction to find that what I sought for, at least by any of these methods of series hitherto delivered in books, was utterly impracticable to any considerable exactness; that is, I take it to be impossible by the method of series and fluxions, as applied by the first inventor's, or other subsequent author's directions, to find the length of one whole quadrant of an ellipsis, whose axes are 2, 1, to ten places. Wolfius, in his second edition of his Elements, (p. 463,) so lately as 1730, bestowed no less than four pages on shewing the process of an operation on the absciss from the centre, but to no manner of purpose; and since he has shewn no other method for those rectifications, but thought

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fit, so very lately, to enlarge that chapter above what it was in the first impression, I conclude that he knew no other, or at least none that he thought better: and whether there be any other at all known, (for what they call evoluta I think will do nothing,) is more than I am able to discover. Trying the parabola I found it worse, and so did T. G.; yet at length, from a close application to the subject, he discovered one fluxional equation of it to be the same with that of the logarithmic; and by substitution of this found means for giving the length of the parabolic line, which by any direct process in the way of series appears impracticable.

Upon the whole, I much question whether it is possible, even by approximation, to rectify any of the conic curves besides the circle, (which by its uniformity in all its parts becomes more easily manageable,) unless by accident; nor am I yet satisfied whether 'tis possible in nature to find a right line equal to any curve whatever.

Therefore as these are my doubts, and in this situation I am entirely out of the way of conferring with the more skilful on these heads, I request, in behalf of thy own art, and, as I may in some sense say, thy profession, to think seriously of the subject, and after a proper inquiry and examination of what is here offered, which I am apt to think will be new, and perhaps somewhat surprising both to thyself and other proficients in geometry, to favour me with thy thoughts on the subject, as soon as may possibly suit thy conveniency, lest, if I am in error, it may prove of ill consequence in another affair, wherein I am led to mention this. I have added, below, the equations I proceeded on in working on Newton's second series,

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which I carried to a considerable length before I discovered its imperfection.

I take the liberty to inclose a print, lately published here, to shew thee how differently I am at times obliged to employ, as I generally do, my thoughts, and am, with sincere respect,

thy assured friend,

J. LOGAN.

The equation for the ellipse is $rx - exx = yy$; x from the vertex

And I find $\dot{z}\dot{z} = \dot{x}\dot{x} \times \frac{rr - 4rex + 4e^2x^2 + 4rx - 4exx}{4rx - 4exx}$.

Dividing this last part I make the quotient $\frac{r}{4x} + 1 - \frac{3}{4}e + \frac{eex}{4r} + \frac{e^3x^2}{4r^2} + \frac{e^4x^3}{4r^3}$, &c. which, after Newton's example, I multiply by rz, to make the first term a square, and then extract, which gives the same that stands in the book, drawing in again $\sqrt{rx}$, dividing by rx, &c. &c.

My series for the arch from the vertex on y.

Equation of the ellipsis $rx - exx = yy$; $r =$ lat. rectum, $e = \frac{r}{\text{Transv. D.}}$.

$$y + \frac{2}{3r^2}y^3 + \left.\begin{matrix} +8e \\ -\frac{2}{5r^4} \end{matrix}\right\} y^5 \left.\begin{matrix} +32e^2 \\ -16e \\ +\frac{4}{7r^6} \end{matrix}\right\} y^7 \left.\begin{matrix} +128e^3 \\ -96e^2 \\ +48e \\ -10 \end{matrix}\right\} y^9 \left.\begin{matrix} +512e^4 \\ -512e^3 \\ +384e^2 \\ -160e \\ +28 \end{matrix}\right\} y^{11}$$

&c. &c.

CIX.

G. ANDERSON TO JONES.

Sir, Twickenham, July 21, 1736.

As you were pleased to shew me a paper of yours for investigating the logarithmic series, I have thereupon taken the liberty to offer you, for your perusal, something I wrote four years ago on the same subject, which I communicated to Mr. Haselden and some more of my mathematical acquaintance.

I was induced to consider the subject anew from reading Dr. Halley's discourse in the Transactions, No. 216, and also the comments that have been written on that method since the Doctor's publication. For though he has been closely followed by them, yet I could not help thinking there were some things that wanted clearing up; as where the Doctor says, "But "if instead of supposing logarithms composed of a "number of ratiunculæ proportional to each ratio, we "shall take the ratio of unity to any number to consist always of the same infinite number of ratiunculæ, "their magnitudes in this case will be as their number "in the former [f]."

It will appear from what follows, that there is no necessity for this new or second idea of logarithms; and what the Doctor adds further, that the logarithm of each ratio is as the fluxion thereof, I think that to be still more difficult to understand. But as to the former assertion, viz. that the magnitudes of the ratiunculæ will be as their number, I demonstrate thus:

[f] Sherwin's Tables, p. 11.

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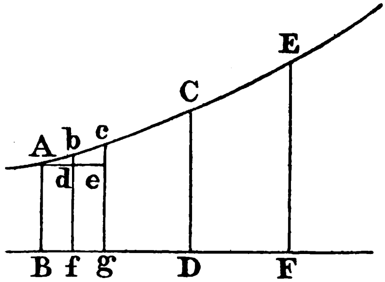

Let ACE be the logarithmic curve, whose asymptote suppose BF, and let AB equal 1, CD $=1+x$, EF $=1+y$, whose logarithms call l, L respectively, then BD $=l$, BF $=L$; and suppose there is an infinite number m of mean proportionals between 1 and $1+x$, and the same infinite number m of proportionals between 1 and $1+y$, and let bf $=1+q$ be the first of the m number of means between 1 and $1+x$, and cg $=1+Q$, the first of the m means between 1 and $1+y$, then bd $=q$, and ce $=Q$, are (in Dr. Halley's style) ratiunculæ: but both bf and cg are very near to AB, because BD and BF are each divided into an infinite number of parts; hence AbcedA may be considered as a right-lined triangle, and therefore dA : eA :: bd : ce; but if m times Bf be equal to l, what is one time Bf equal to, which is $\frac{l}{m}$, for $m : l :: 1 : \frac{l}{m} =$ Bf $=$ dA? and for the same reason $m : L :: 1 : \frac{L}{m} =$ Bg $=$ eA; hence the former proportion dA : eA :: bd : ce gives $\frac{l}{m} : \frac{L}{m} :: q : Q$, that is, $l : L :: q : Q$, which is the Doctor's proposition. Q. E. D.

To come to the method I propose; let us suppose an infinite number $l-1$ of mean proportionals between 1 and $1+x$, and put $1+q$ for the first of them; hence the means are $1+q$, $\overline{1+q}^{\,2}$, $\overline{1+q}^{\,3}$, &c. to $\overline{1+q}^{\,l-1}$, and the series itself 1, $\overline{1+q}^{\,1}$, $\overline{1+q}^{\,2}$, $\overline{1+q}^{\,3}$, &c. to $\overline{1+q}^{\,l}=1+x$, therefore 0, 1, 2, 3, &c. to l are the logarithms of 1, $1+q$, $\overline{1+q}^{\,2}$, $\overline{1+q}^{\,3}$, &c. to $1+x$, or

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the logarithms of the ratio of unity to those numbers: hence since $\overline{1+q}|^{l}=1+x$, $1+q=\overline{1+x}|^{\frac{1}{l}}$, or $1+q=1+\frac{1}{l}x+\frac{1}{l}\times\frac{\frac{1}{l}-1}{2}x^2+\frac{1}{l}\times\frac{\frac{1}{l}-1}{2}\times\frac{\frac{1}{l}-2}{3}x^3$, &c.; but l being infinitely great, $\frac{1}{l}$ is infinitely small; hence $\frac{1}{l}-1=-1$, $\overline{\frac{1}{l}-1}\times\overline{\frac{1}{l}-2}=-1\times-2=2$, &c.; therefore $1+q=1+\frac{1}{l}x-\frac{1}{2l}x^2+\frac{1}{3l}x^3-$ &c. or $q=\frac{1}{l}x-\frac{1}{2l}x^2+\frac{1}{3l}x^3$, &c. or $lq=x-\frac{1}{2}x^2+\frac{1}{3}x^3$,&c.; therefore $l=\frac{1}{q}\times\overline{x-\frac{1}{2}x^2+\frac{1}{3}x^3}$, &c. Q. E. I.

And hence we may observe, the logarithms shall be different, according to the different values of q, which is evident from the nature of the thing, as will appear by considering the above figure.

Therefore to what purpose can it be to introduce a new property of logarithms, whose truth might be questioned, since the Doctor has given us no demonstration of it; especially since the known one, (viz. that logarithms are as the number of terms or ratiunculæ between the terms of the ratio,) gives the series without any other assistance than what the Doctor uses in the above-cited Transaction? I can see no reason, and therefore am willing to submit it to your better judgment.

Thus the logarithmic series is determined without the assistance of the method of fluxions; but if any are desirous to apply those principles, they may proceed thus. Let us assume our form or equation $\overline{1+q}|^{l}=1+x$, and let l and x flow in the same infinitely small particle of time to $l+o\dot{l}$ and $1+x+o\dot{x}$, then

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$\overline{1+q}^{\,l+o\dot{l}} = 1 + x + o\dot{x}$; divide this equation by the former, then $\overline{1+q}^{\,o\dot{l}} = 1 + \frac{o\dot{x}}{1+x}$; but $\overline{1+q}^{\,o\dot{l}} = 1 + o\dot{l}q + o\dot{l} \times \frac{o\dot{l}-1}{2} q^2 + o\dot{l} \times \frac{o\dot{l}-1}{2} \times \frac{o\dot{l}-2}{3} q^3$, &c. But $o\dot{l}$ is infinitely small by supposition, therefore $\frac{o\dot{l}}{2} - \frac{1}{2} = -\frac{1}{2}$, and $\frac{o\dot{l}}{3} - \frac{2}{3} = -\frac{2}{3}$; hence the series becomes $\overline{1+q}^{\,o\dot{l}} = 1 + o\dot{l}q - \frac{o\dot{l}q^2}{2} + \frac{o\dot{l}q^3}{3}$ – &c.: but here again q is a very small quantity, therefore $\frac{q^2}{2}, \frac{q^3}{3}$, are nothing with relation to $1 + o\dot{l}q$, thence we get $\overline{1+q}^{\,o\dot{l}} = 1 + \frac{o\dot{x}}{1+x} = 1 + o\dot{l}q$, or $\dot{l}q = \frac{\dot{x}}{1+x} = \dot{x} - x\dot{x} + x^2\dot{x}$, &c. Therefore, (because q is given,) $lq = x - \frac{1}{2}x^2 + \frac{1}{3}x^3$, &c.; and hence $l = \frac{1}{q} \times \overline{x - \frac{1}{2}x^2 + \frac{1}{3}x^3}$, &c.

I proposed to have waited on you before I left London, but was obliged to depart after so short a warning, that I could not have the pleasure; so got my lord Craufurd's gentleman to deliver this to you, that so you might favour me with your opinion when I come to town, which I expect will be on Friday next, when I shall take the liberty to come and offer my respects, being, with great esteem, Sir,

your very humble and very devoted servant,

G. ANDERSON.

The cover of this letter, with the address, has not been preserved; but there can be no doubt of its being intended for

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Jones, whose name appears on another letter from the same writer: the allusion likewise to Jones's paper on logarithms, (which was probably the same that was afterward published, Ph. Trans. vol. LXI. p. 455,) confirms this conclusion.

The mention of lord Craufurd's servant seems to indicate G. Anderson to have been some one in an humble station of life; and although his handwriting is tolerably good, his spelling and the grammar have occasionally required correction.

CX.

MACHIN TO JONES.

Dear Sir, July 30, 1736.

I was under some concern at the dissatisfaction which you expressed at that little slip of paper, which you had of me, by reason I thought it contained something that was better than ordinary of the kind. And I could not be at ease till I drew up an account, such as will satisfy you of the truth of the theorem, and I believe of the remark too at the end of it, unless the hurry I was in made me forget to except the use of the binomial theorem, which you can't but remember I have always acknowledged as absolutely necessary in the resolutions of problems. You will see by the propositions, which I send you, that the invention is founded on a thought I have mentioned to you of dividing figures into polygons, without which I am apt to think it would not be obvious to find an algebraic equation, and that only of three dimensions, which will serve to assign nearly the area of the whole circle, as well as any part of it, and which in this case is necessary. As I was put to the necessity of giving some account of a new or rather very old method, I could not comprehend it in less than near three sheets of paper, being unwilling

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to write the several theorems in too close a writing. But if it should so happen that there is no convenient way for such a packet to be conveyed to you, I have ordered the maid, who carries it, to tell your folks that she will call for it again in about a week, that I may take a copy of it, which will be somewhat troublesome to do out of my loose papers.

My sister being very ill, I dare not stir from the place, though at a great charge, especially since I find some difficulties in removing in the manner I intended. But I shall not trouble you now with the state of my affairs, but only say,

I am

your much obliged humble servant,

J. Machin.

CXI.

MACHIN TO JONES.

Dear Sir, August 5, 1736.

I am very glad you received any manner of satisfaction from the paper I sent you. Bernoulli[g] did print some theorem for the tangent, which if you remember is the same as was sent him from hence. One for the cosine is also printed in some shape or other by Mr. Demoivre, in his Miscellanea, who demonstrates it from properties of the hyperbola, which were known long before the method of fluxions. I derive them from the general doctrine of ratios, whereof I not long since gave you some hint. 'Tis no concern of mine who was the first author of them, or any of them, or whether the first making such theorems was taken from that of the tangent or not, though I believe

[g] See Jo. Bernoulli Opera, vol. IV. p. 98.

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it was. The method by polygons is demonstrative and universal, and entirely distinct from the method of fluxions in the nature of the thing. For, in the example I gave, you see there is no rule of fluxions or fluents made use of in finding the quadrature; and so it is in all other cases, excepting, that when the curve is perfectly quadrable, the same rules arise as are given in the other method, but not for the same reason. There is no knowledge nor art, requisite to understand or make out the truth of these rules, more than what is found in the fifth book of Euclid, nor any reasoning but what is intelligible to any one of plain sense and understanding: it extends to all quadratures that are yet known, and is truly demonstrable, because founded on this incontestable principle, that every figure is bigger than that which it includes, and less than that in which it is included. This I do not scruple to call an axiom, and yet it is directly and in terms contradictory to a proposition pretended to be demonstrated by a great author, two of whose champions are now quarrelling with one another about which of them it is that understands it best; though neither of them has the ingenuity to own or sense to perceive that it is not true. But of this enough.

The kind concern which you express upon my affair cannot but be extremely grateful to me; but you are to know that my philosophy does not prove altogether unuseful to me; for though my place, and perhaps the opinion of others about me, may be changed, I am not changed, and have taken a resolution of making a virtue out of necessity. Even this retirement and condition has happened to put that within my power, which was out of my power before, and which was the only thing I wanted to acquit myself in every undertaking. For it has gored me to think that I was

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perpetually liable to a just charge of never finishing any thing, without being able to explain how that came to pass. Now since I have been here I have been able to recollect my broken thoughts concerning this method of quadrature, and see that I have time enough without interruption to put them together, and to comprise it all in a short compass, and to get it printed; by which at least I shall obtain two ends, one, to shew that I have some tolerable skill in sifting out the principles of a dark art, and another will be to clear myself from the unjust and malicious aspersions, without the least ground, of that empty hare-brained author, the Minute Mathematician, who, like the fool in the Proverbs, scatters his firebrands, and says, "Am I not in sport?" and who, like a madman in his raving fits, bites those who are entire strangers to him and his cause and who never gave him any manner of provocation. I am thoroughly convinced that when the true demonstration of the things, his favourite method aims at, shall once appear, his consummate vanity in talking, like a parrot, by rote, upon things wherein he is as a philosopher totally ignorant, must expose him and his anti-associate to the scorn of all that know 'em. But I trouble you no more at present: I question not but that I shall quickly be able to acquit myself in this, at least, if I shall have time for nothing further, in a public manner to all my friends, and especially to, dear Sir,

your most obliged friend
and humble servant,
J. MACHIN.

P. S. My sister being extremely ill, I dare not for that reason only, were there no other, remove further from home.

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CXII.

ANDERSON TO JONES.

Sir,

You have here the investigation of a series invented by Mr. Cunn for finding the periphery of an ellipse, which though 'tis nothing more than an easy cor. to the seventh proposition of the Book of Quadratures, yet I can't find that any body hitherto has lit on a method to investigate it. I have also sent you an investigation of the first case of the above proposition, (as being all that pertains to the present series,) and which I used, before I had seen that book, in demonstrating Mr. De Moivre's six theorems in the Transactions, No. 278.

Let A, B, C, D, E, &c. be the areas of curves, whose ordinates are $z^{\theta-1} R^{\lambda-1}$, $z^{\theta+\eta-1} R^{\lambda-1}$, $z^{\theta+2\eta-1} R^{\lambda-1}$, $z^{\theta+3\eta-1} R^{\lambda-1}$, $z^{\theta+4\eta-1} R^{\lambda-1}$, &c. respectively, where $R = e + fz^{\eta} + gz^{2\eta} + hz^{3\eta} + iz^{4\eta}$ &c.; consequently,

First Series,	and assume the Second Series,
$\dot{A} = z^{\theta-1}\dot{z} R^{\lambda-1}$	$A = \dfrac{z^{\theta} R^{\lambda-1}}{\theta} - a$
$\dot{B} = z^{\theta+\eta-1}\dot{z} R^{\lambda-1}$	$B = \dfrac{z^{\theta+\eta} R^{\lambda-1}}{\theta+\eta} - b$
$\dot{C} = z^{\theta+2\eta-1}\dot{z} R^{\lambda-1}$	$C = \dfrac{z^{\theta+2\eta} R^{\lambda-1}}{\theta+2\eta} - c$
$\dot{D} = z^{\theta+3\eta-1}\dot{z} R^{\lambda-1}$	$D = \dfrac{z^{\theta+3\eta} R^{\lambda-1}}{\theta+3\eta} - d$
$\dot{E} = z^{\theta+4\eta-1}\dot{z} R^{\lambda-1}$	$E = \dfrac{z^{\theta+4\eta} R^{\lambda-1}}{\theta+4\eta} - e$
&c.	&c.

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therefore Third Series

$$\dot{a} = \frac{\lambda - \iota}{\theta} z^{\theta} R^{\lambda-2} \dot{R}$$

$$\dot{b} = \frac{\lambda - \iota}{\theta + \eta} z^{\theta+\eta} R^{\lambda-2} \dot{R}$$

$$\dot{c} = \frac{\lambda - \iota}{\theta + 2\eta} z^{\theta+2\eta} R^{\lambda-2} \dot{R}$$

$$\dot{d} = \frac{\lambda - \iota}{\theta + 3\eta} z^{\theta+3\eta} R^{\lambda-2} \dot{R}$$

$$\dot{e} = \frac{\lambda - \iota}{\theta + 4\eta} z^{\theta+4\eta} R^{\lambda-2} \dot{R}$$

&c.

consequently Fourth Series

$$\dot{a} z^{\eta} = \frac{\theta + \eta}{\theta} \dot{b}$$

$$\dot{a} z^{2\eta} = \frac{\theta + 2\eta}{\theta} \dot{c}$$

$$\dot{a} z^{3\eta} = \frac{\theta + 3\eta}{\theta} \dot{d}$$

$$\dot{a} z^{4\eta} = \frac{\theta + 4\eta}{\theta} \dot{e}$$

&c.

But since $\dot{a} = \frac{\lambda - 1}{\theta} z^{\theta} R^{\lambda-2} \dot{R}$, therefore $\dot{a} \times R = \dot{a}e + \dot{a}fz^{\eta} + \dot{a}gz^{2\eta} + \dot{a}hz^{3\eta} + \dot{a}iz^{4\eta}$&c. $= \frac{\lambda - 1}{\theta} z^{\theta} R^{\lambda-1} \dot{R}$; for $\dot{R}$ put its value $\eta f z^{\eta-1}\dot{z} + 2\eta g z^{2\eta-1}\dot{z} + 3\eta h z^{3\eta-1}\dot{z} + 4\eta i z^{4\eta-1}\dot{z}$ &c. and then we get the following fluxional equation:

$$\dot{a}e + \dot{a}fz^{\eta} + \dot{a}gz^{2\eta} + \dot{a}hz^{3\eta} + \dot{a}iz^{4\eta} \text{ \&c.} = \overline{\lambda - 1} \times$$

$$\frac{\eta}{\theta}\sqrt{fz^{\theta+\eta-1}\dot{z}R^{\lambda-1} + 2gz^{\theta+2\eta-1}\dot{z}R^{\lambda-1} + 3hz^{\theta+3\eta-1}\dot{z}R^{\lambda-1} + 4iz^{\theta+4\eta-1}\dot{z}R^{\lambda-1} \text{ \&c.}}$$

Find the fluent of the first side of the equation by the fourth series, and of the second part by the first series, and then we get the following equation:

$\theta ae + \overline{\theta + \eta} \times fb + \overline{\theta + 2\eta} \times gc + \overline{\theta + 3\eta} \times hd + \overline{\theta + 4\eta} \times ie$ &c. $= \overline{\lambda - 1} \times \eta\sqrt{fB + 2gC + 3hD + 4iE,}$ &c.; hence if for $a, b, c, d, e,$ &c. be put their values from the second series, and also in the expression so coming out for $e + fz^{\eta} + gz^{2\eta}$ &c. put R, and we get this final equation $z^{\theta}R^{\lambda} - eA\theta = \overline{\lambda\eta + \theta} \times fB + \overline{2\lambda\eta + \theta} \times gC + \overline{3\lambda\eta + \theta} \times hD +$

$4\overline{\lambda\eta+\theta}\times iE$, &c. and hence, if the curve be of the binomial kind and consequently g, h, i, &c. $=o$, then $B=\dfrac{z^{\theta}R^{\lambda}-\theta eA}{\overline{\lambda\eta+\theta}\times f}$.

Corollary.

If there be a series of curves, whose ordinates are $\dfrac{x^m}{\sqrt{r^2-x^2}}, \dfrac{x^{m-2}}{\sqrt{r^2-x^2}}, \dfrac{x^{m-4}}{\sqrt{r^2-x^2}}, \dfrac{x^{m-6}}{\sqrt{r^2-x^2}}$, &c. to $\dfrac{x^{m-2\eta}}{\sqrt{r^2-x^2}}$ and their areas $A, \alpha, \beta, \gamma, \delta$, &c. to B, then put $y=\sqrt{r^2-x^2}$

$$A=r^2\alpha\times\frac{m-1}{m}-\frac{1}{m}x^{m-1}y$$

$$\alpha=r^2\beta\times\frac{m-3}{m-2}-\frac{1}{m-2}x^{m-3}y$$

$$\beta=r^2\gamma\times\frac{m-5}{m-4}-\frac{1}{m-4}x^{m-5}y$$

$$\gamma=r^2\delta\times\frac{m-7}{m-6}-\frac{1}{m-6}x^{m-7}y$$

If for α in the first step its value in the second be put, and in the expression so coming out, for β be put its value and so on to B, we shall find that $A=r^{2\eta}\times B\times\dfrac{m-1}{m}\times\dfrac{m-3}{m-2}\times\dfrac{m-5}{m-4}$ &c. $\dfrac{m-2n+1}{m-2n+2}-\dfrac{1}{m}x^{m-1}y-\dfrac{r^2}{m-2}\times\dfrac{m-1}{m}x^{m-3}y-\dfrac{r^4}{m-4}\times\dfrac{m-1}{m}\times\dfrac{m-3}{m-2}\times x^{m-5}y$, and so on to the last term, which will be $\dfrac{r^{2\eta-2}}{m-2n+2}\times\dfrac{m-1}{m}\times\dfrac{m-3}{m-2}\times\dfrac{m-5}{m-4}\times\dfrac{m-7}{m-6}$ &c. to $\dfrac{m-2n+3}{m-2n+4}$, which is Mr. De Moivre's fourth theorem

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on the foresaid transaction. And hence, if $r=x$, then $A=r^{2n}B\times\frac{m-1}{m}\times\frac{m-3}{m-2}\times\frac{m-5}{m-4}\times\frac{m-7}{m-6}$ &c. to $\frac{m-2n+1}{m-2n+3}$.

In the adjacent figure (Plate 3, fig. 2) put CD$=x$, DF$=y$, AC$=r$, CE$=c$; then from the property of the ellipse $r^2y^2=r^2c^2-c^2x^2$, therefore $\dot{y}=-\frac{c^2x\dot{x}}{r^2y}$ and $\dot{y}^2=\frac{c^4x^2\dot{x}^2}{r^2\times\overline{r^2-x^2}}$; therefore put $E=$arch EF, $C=$arch GH. Then $\dot{E}=\dot{x}\sqrt{\frac{r^4-r^2x^2+c^2x^2}{r^2\times\overline{r^2-x^2}}}$, put $e^2=\frac{r^2-c^2}{r^2}$ then $\dot{E}=\dot{x}\sqrt{\frac{r^2-e^2x^2}{r^2-x^2}}$; when $r=c$ *or* $e=$o, then the ellipse becomes a circle, hence $\dot{C}=\frac{r\dot{x}}{\sqrt{r^2-x^2}}$. Let $\sqrt{r^2-e^2x^2}$ be thrown into a series, then $\dot{E}=\frac{r\dot{x}}{\sqrt{r^2-x^2}}-\frac{e^2x^2\dot{x}}{2r\sqrt{r^2-x^2}}-\frac{e^4x^4\dot{x}}{8r^3\sqrt{r^2-x^2}}-\frac{3e^6x^6\dot{x}}{48r^5\sqrt{r^2-x^2}}-\frac{15e^8x^8\dot{x}}{384r^7\sqrt{r^2-x^2}}$. Let each term of this series be compared with $\dot{C}=\frac{r\dot{x}}{\sqrt{r^2-x^2}}$, then by the above theorem when $r=x$, put $m=2, n=1$, the fluent of $-\frac{e^2x^2\dot{x}}{2r\sqrt{r^2-x^2}}=-\frac{e^2}{2r^2}\times r^2C\times\frac{m-1}{m}=-\frac{e^2}{4}C$; put $m=4$, $n=2$, the fluent of $-\frac{e^4x^4\dot{x}}{8r^3\sqrt{r^2-x^2}}=-\frac{e^4}{8r^4}\times r^4C\times\frac{m-1}{m}\times\frac{m-3}{m-2}=-\frac{3e^4C}{64}$; put $m=6, n=3$, the fluent

of $-\dfrac{3e^6x^6\dot{x}}{48x^5\sqrt{r^2-x^2}} = \dfrac{3e^6}{48r^6}\times r^6\times C\times\dfrac{m-1}{m}\times\dfrac{m-3}{m-2}\times$ $\dfrac{m-5}{m-4} = -\dfrac{45e^6C}{2304}$; put $m=8$, $n=4$, the fluent of $-$ $\dfrac{15e^8x^8\dot{x}}{384x^7\sqrt{r^2-x^2}} = -\dfrac{15e^8}{384r^8}\times r^8\times C\times\dfrac{m-1}{m}\times\dfrac{m-3}{m-2}\times$ $\dfrac{m-5}{m-4}\times\dfrac{m-7}{m-6} = -\dfrac{1575e^8C}{147456}$. But if the first term be called α, the second β, the third γ, &c. then $E=C+$ $\dfrac{-1\times1}{2\times2}\alpha e^2+\dfrac{1\times3}{4\times4}\beta e^2+\dfrac{3\times5}{6\times6}\gamma e^2+\dfrac{5\times7}{8\times8}\delta e^2$, &c. which is the series sought.

If you think any thing herein merits a place in the Transactions, you would highly oblige me to hint at the method I ought to proceed therein; but, however that may be, my greatest ambition is accomplished from the occasion it gives me to assure you

I am, with great esteem,

Sir, your very humble

and most obedient servant,

GEO. ANDERSON.

Twickenham,
Aug. 28, 1736.

P. S. Having often hinted a theorem for equation of payments, I shall here transcribe it. Put D, d, two debts due T, t, times hence, r the rate of interest, and x the time sought, then $x=\dfrac{TD+td}{D+d}$ sim. int. and $x=$ $\dfrac{L.\,\overline{D+d}-L.\,\overline{d\times\overline{1+r}|^{T-t}+D}+T\times L.\,\overline{1+r}}{L.\,\overline{1+r}}$ compound interest.

CXIII.

G. ANDERSON TO JONES.

Sir,

You have here the investigation of the eleventh proposition of the book of Quadratures I was speaking to you of, which I fancy is somewhat shorter than Mr. Robins's[a], though I dare not hope it comes up to the elegancy of yours.

I need not repeat the proposition, but only observe, for Sir Isaac's A, B, C, D, &c., I put T, $\underset{1}{T}$, $\underset{2}{T}$, $\underset{3}{T}$, &c., and for the areas AEKC, AFLC, AGMC, &c., I put λ, $\underset{1}{\lambda}$, $\underset{2}{\lambda}$, $\underset{3}{\lambda}$, &c., where the underwrit figures are to be understood as in page 1 of Mr. Craig De Calculo Fluentium.

Hence, from the proposition, and the common principles of quadratures, I have the following series:

$$\dot{T} = y\dot{z} \qquad \dot{\lambda} = T\dot{z}$$

$$\underset{1}{\dot{T}} = zy\dot{z} \qquad \underset{1}{\dot{\lambda}} = \lambda\dot{z}$$

$$\underset{2}{\dot{T}} = z^2y\dot{z} \qquad \underset{2}{\dot{\lambda}} = \underset{1}{\lambda}\dot{z}$$

$$\underset{3}{T} = z^3y\dot{z} \qquad \underset{3}{\dot{\lambda}} = \underset{2}{\lambda}\dot{z}$$

&c. to   &c. to

$$\underset{\eta}{\dot{T}} = z^{\eta}y\dot{z} \qquad \underset{\eta}{\dot{\lambda}} = \underset{\eta-1}{\lambda}\dot{z}$$

[a] Phil. Trans. vol. xxxiv. p. 230, and Robins's Tracts, vol. ii. p. 168.

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$$\dot{P} = y\dot{z} = y\dot{z} = y\dot{z}$$

$$\dot{Q} = xy\dot{z} = \overline{t - z}.y\dot{z} = ty\dot{z} - zy\dot{z} \text{ (for } t - z = x)$$

$$\dot{R} = x^2y\dot{z} = \overline{t - z}|^2 y\dot{z} = t^2y\dot{z} - 2\,tzy\dot{z} + z^2\,y\dot{z}$$

$$\dot{S} = x^3y\dot{z} = \overline{t - z}|^3 y\dot{z} = t^3y\dot{z} - 3\,t^2zy\dot{z} + 3\,tz^2y\dot{z} - z^3y\dot{z}.$$

The fluent of each term of the third series may be found from the first series by inspection, (for t is constant;) therefore $P = T$, $Q = t\,T - T_1$, $R = t^2T - 2\,tT_1 + T_2$, $S = t^3T - 3\,t^2T_1 + 3\,tT_2 - T_3$; and this is one part of the prop.

N.B. $\dot{z}$ is the fluxion of the absciss in all the series, since one ends where the other begins.

For the other part, or to find the fluents of $\dot{\lambda}$, $\dot{\lambda}_1$, $\dot{\lambda}_2$, $\dot{\lambda}_3$, &c., I premise this lemma. The fluent of $z^r\dot{z}\,T_s$ is $\dfrac{z^{r+1}T_s - T_{r+s+1}}{r+1}$; for the fluxion thereof, is $z^r\dot{z}\,T_s + \dfrac{z^{r+1}\dot{T}_s - \dot{T}_{r+s+1}}{r+1}$; but $\dot{T}_s = z^s y\dot{z}$ therefore $z^{r+1}\dot{T}_s = z^{r+s+1}y\dot{z}$, and the fluent thereof is T_{r+s+1}, as is manifest from the first series; and hence the thing is evident.

But now we may find the fluents of $\dot{\lambda}$, $\dot{\lambda}_1$, $\dot{\lambda}_2$, $\dot{\lambda}_3$, &c., by inspection, for $\dot{\lambda} = T\dot{z}$; here $r = 0$, $s = 0$, therefore per lem. $\lambda = z\,T - T_1$; again, $\dot{\lambda}_1 = \lambda\,\dot{z} = z\dot{z}\,T - \dot{z}\,T_1$; in the lem. first write 1.0, and then 0.1 for r, s,

and we get $\lambda_1 = \frac{z^2T - T_2}{2} - \frac{zT_1 + T_2}{1} = \frac{z^2T - 2zT_1 + T_2}{2}$;

again, $\dot{\lambda}_2 = \lambda_1 \dot{z} = \frac{z^2\dot{z}T - 2z\dot{z}T_1 + \dot{z}T_2}{2}$, and therefore in the first place $r = 2$, $s = 0$; secondly $r = 1$, $s = 1$; thirdly $r = 0$, $s = 2$, therefore $\lambda_2 = \frac{z^3T - T_3}{6} -$

$\frac{z^2T_1 + T_3}{2} + \frac{zT_2 - T_3}{2} = \frac{z^3T - 3z^2T_1 + 3zT_2 - T_3}{6}$,

and so on to infinity: write t for z, and then we have Sir Isaac's expression. Q. E. O.

Though I dread to have tired you already, yet I beg your patience a little longer to look over an investigation for finding the fulcrum of a balance, wherein the weight of the beam is considered; the shape of the beam is the frustum of a square pyramid; M, N (Plate 3, fig. 3.) are the centres of gravity of the frustums DL, DK, respectively.

Put AB $= l$, BK $= b$, AL $= a$, BC $= x$, AM $= g$, BN $= G$, DK $= F$, DL $= f$, vidt., their solidities, $a =$ solidity of the beam, and m its weight.

Then $a : m :: F : \frac{mF}{a} =$ weight DK; and $a : m :: f : \frac{mf}{a} =$ weight DL; again $x : x - G$ (=CN) :: $\frac{mF}{a} : \frac{mF}{ax} \times \overline{x - G} =$ to the momentum of DK at B, and from the same principle $l - x : l - x - g$ (=CM) :: $\frac{mf}{a} : \frac{mf}{a \times \overline{l - x}} \times \overline{l - x - g} =$ to the momentum of DL at

A; hence $\frac{mF}{ax}\times\overline{x-G}+W$ is the whole momentum or force acting at B, and $\frac{mf}{a\times\overline{l-x}}\times\overline{l-x-g}+w$ is that acting at A: for W, w are the weights suspended from B, A, respectively; and hence, since they are in equilibrium by supposition, we have $x : l-x ::$ $\frac{mf}{a\times\overline{l-x}}\times\overline{l-x-g}+w : \frac{mF}{ax}\times\overline{x-G}+W$ and from hence this equation $mf\times\overline{l-x-g}+aw\times\overline{l-x}=$ $mF\times\overline{x-G}+aWx$. For f put its value $a-F$, and we have $mal-max-mFl+\overline{F-a}\times mg+wa\times$ $\overline{l-x}=Wax-mFG$. Next to find g; $a-F=f$, fl. $\frac{\dot{F}x}{F}=G$, fl. $\frac{\dot{f}y}{f}=g$, from the rules to find the centres of gravity, therefore $-\dot{F}=\dot{f}$, therefore $\dot{f}y=-$ $\dot{F}y=-\dot{F}\times\overline{l-x}=-\dot{F}l+\dot{F}x$; but fl. $\dot{F}l=Fl$, and fl. $\dot{F}x=F\times G$ (for fl. $\frac{\dot{F}x}{F}=G\therefore$ fl. $\dot{F}x=F\times G$,) hence $g=\frac{F\times G-Fl}{a-F=f}$, exact except the standing quantities that are to be added: we will call them Q; hence $g=\frac{Q+F\times G-Fl}{a-F}$; put this for G in the above equation, then we have $mal-max-mFl-mQ-$ $mFG+mlF+wal-wax=Wax-mFG$, and from hence $mal-max-mQ+wal-wax=Wax$, therefore $x=\frac{\overline{m+w}\times al-mQ}{\overline{m+w+W}\times a}$, where every thing is known but Q, to find which let us reassume the value of $g=\frac{Q+FG-Fl}{a-F}$; here when $F=0$, then $g=\frac{Q}{a}$ is

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the distance of the centre of gravity of the whole beam from A; so now we must find that distance. Per fig. $l : b - a : y : \frac{by - ay}{l} = \mathrm{DR}$, therefore $\frac{by - ay + al}{l} =$ DC, hence the fluent $-\frac{4}{l^2} \times \frac{\overline{by - ay + al}|^2}{f} \cdot \dot{y}y = g =$ $\frac{\overline{3b^2 + 2ba + a^2} \times l^2}{3a}$ when $f = a$ or $F = 0$, hence $\frac{Q}{a} =$ $\frac{\overline{3b^2 + 2ba + a^2} \times l^2}{3a}$, therefore $Q = \frac{\overline{b + a}|^3 + 2b^3}{3} \times l^2$: put this for Q in the above value of x, then we get $x =$

$$\frac{\overline{\overline{m + w} \cdot 3a - ml \times \overline{\overline{b + a}|^2 + 2b^2}} \times l.}{3a \times \overline{m + w + W}}$$

If the beam is a parallelopipedon, then $x = \frac{\overline{m + 2w} \times l}{2m + 2w + 2W}$.

If you should happen to have a solution of the above problem, I beg you'll favour me with it, at least with your opinion of mine, because I have heard of a theorem for the same purpose different from the above. I have ordered the footman to call, at his coming away, and receive your commands.

I am, Sir,

your most devoted and

very humble servant,

GEO. ANDERSON.

Twickenham,
Oct. 28, 1736.

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CXIV.

ANDERSON TO JONES.

Sir,

Among the various applications of the method of fluxions to mathematical problems, I don't find that any of the writers thereon ever gave the areas of spherical triangles by a fluxionary process. Wherefore, having (as I conceive) obtained a very easy and natural solution thereof, I judged you would not be displeased with the investigation.

Let ABC (Plate 3, fig. 4,) be any spherical triangle, in which CB and the angle C may be considered as permanent quantities, but the rest of the parts as flowing; and call the angles ABC, ACB, BAC, e, x, z, respectively, the side CA $= y$, its sine $s.\ y$, cosine $c.\ y$, and put $a =$ area of the triangle sought, radius r, $p =$ circumference of a circle whose diameter is unity.

Now the sides AB, AC, CA, CB being produced to D, E, G, F, so that AD, AE, CG, CF may be quadrants, and also through C the great circle mCbaH being drawn infinitely near to ECAG, it is manifest from the elements that ED $= z$, Em $= \dot{z}$, FG $= x$, GH $= \dot{x}$, and lastly through A let the small circle bAnb be drawn parallel to HGF, then because abA is infinitely small in respect of CAa, the fluxion of the triangle BCA is $\dot{a} =$ CAb.

From the known methods, the circumference of the circle bAnb is $2p \times s.y$ and the surface of the whole spherical segment CbAnb is $2pr \times \overline{r - c.\ y}$, but the areas of any parts of that segment, made by great circles passing through the pole C, are as the arches of the parallel on which they stand, that is, $2p \times s.\ y : 2pr \times$

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$\overline{r-c.y}::\text{bA}:r.\frac{\overline{r-c.y}}{s.y}\times\text{bA}=\dot{a}=\text{CAb}$; but $r:s.y::\dot{x}$ (HG); $\frac{s.y\times\dot{x}}{r}=\text{bA}$, hence $\dot{a}=\overline{r-c.y}\times\dot{x}=r\dot{x}-c.y\times\dot{x}$. But because AD, AE are quadrants, A is the pole of the circle EmD, and therefore the angle CEm is a right angle, and so is CmE because b is infinitely near A; but in all spherical triangles the sines of the sides are as the sines of their opposite angles, therefore s. mEC = s. CmE(r): $s.$ CE $(c.y)::s.$ECm $(s.\dot{x}):s.$ Em $(s.-\dot{z})$, but the sines of very small arches are as the arches themselves, hence $s.\dot{x}:s.-\dot{z}::\dot{x}:-\dot{z}$, wherefore $r:c.y::\dot{x}:-\dot{z}$, or $r\dot{z}=-\dot{x}\times c.y$, but $\dot{a}=r\dot{x}-\dot{x}\times c.y$, therefore $\dot{a}=r\dot{x}+r\dot{z}$ and $a=rx+rz+q$.

When CA coincides with CB, then both a and x become $=0$ and z becomes CBD $=180^\circ-e$, therefore in that case $a=0=r\times0+r\times\overline{180^\circ-e}+q$ or $q=-r\times\overline{180^\circ-e}$, whence lastly $a=rx+rz+q=rx+rz-r\times\overline{180^\circ-e}=\overline{x+z+e-180^\circ}\times r$.

It is my greatest ambition to have my little performance approved of by you; but nevertheless should this fail of the desired success, yet I should not altogether miss my aim, because of the occasion it gives me to assure you,

I am, with great esteem,

your very humble and devoted servant,

G. Anderson.

P.S. I called at your house last week with Stirling's book; but not having the fortune to meet you at home, I left it with your servant, and have since had the good luck to meet with one, which I bought.

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CXV.

LOGAN TO JONES.

My good friend,

I am favoured with thine of the 20th of February, but cannot say whether more to my satisfaction on the one hand, or to my confusion on the other; to the first it was greatly, nor could it be less to the other, to observe what trouble, without any just claim on my part to the favour, I had given thee. I have, however, by it the most pregnant instances of thy goodness, thy reflection on which may also yield a sincere satisfaction to thyself, as nothing but a large share of benevolence, the highest of human virtues, could have led thee to such a condescension. For I cannot omit remarking that to have shewn thy method for rectifying that curve (the ellipse) would have been abundantly sufficient, without proceeding to the operation itself, which I perceive from its slow convergency must have been unreasonably operose to carry it even to ten figures: for I find, after a few steps, the decrease in the terms of the series will, for that ratio of diameters (2:1), be but little more than $\frac{1}{4}$ in each, and to come at those ten figures the series must, if I mistake not, be carried to above eighty places. My obligation is therefore the greater. But the fineness of the invention, the neatness of the demonstration, with the exactness and plainness of the process, still add highly to the pleasure; and, upon the whole, I must with sincere gratitude ever acknowledge myself very much thy debtor.

From what I touched on in these mathematical subjects, I suppose it was not difficult for thee to take a

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pretty exact measure of my small skill in them. 'Tis what I never pretended to; nor did I ever look into them, otherwise than now and then for my diversion, and because I would not be wholly ignorant of those sciences, that I was assured, on all hands, were both highly useful and entertaining; and therefore, though without the instruction of any master, I resolved to get some little acquaintance with them. But my life has been generally a course of business, and the hours I could borrow from that have been employed in a continued variety, I cannot say of study, but of amusement from books promiscuously; and it was but by accident that I looked into the method of series and fluxions applied to them, which I had avoided through an apprehension of their being too intricate: but endeavouring (as I think I formerly hinted) to find the length of an elliptic curve, the proportion of which to that of the circle, considering their sines are exactly in the same ratio and differ only by the prolongation of one axis, I thought might easily be discovered; but trying several methods for it, and still finding myself disappointed, I resolved to try it by that method of series which I thought must infallibly give it me, and accordingly, with some little application, made myself master of those given by Newton in his letter of July[a], 1676, drawn up for Leibnitz.

His first series for x from the centre proved easy, and much the more so, because he gave another for continuing it; his other for x from the vertex, as he had found himself obliged to use much more contrivance in it, gave me more trouble, and still the more, because I could not find any possible method for continuing it without actually working it. By that means,

[a] It should be June, see Com. Ep. p. 131, on sheet K; for K and L are both paged 129—144.

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however, I struck out a canon of my own, which exceedingly facilitated that and all other such operations. Having gained this, and carried the series a good many steps further, I then concluded I was absolutely master of the whole; but when I came to reduce it to numbers, behold! I was just as far off my point as ever; the series after a very few steps actually diverged and ran still wider. I then threw away my whole work with indignation; and with no small amazement to find it possible for Sir Isaac Newton to commit such a blunder. I examined, however, my own work, as I also did his, over and over, and at length I discovered what he had overlooked, the true source of the error, as I mentioned it to thee (with more, I think, of what I am writing) in a former letter; that is, that the second or third term in the series, whose root was extracting, was greater in value than the preceding; a circumstance that, if I mistake not, will destroy the convergency in any extraction whatever. But of this, I observed, that great man was not at all sensible at the time he was writing that letter, for in his very second example of extraction, which is to find $\overline{c^5+c^4x-x^5,}|^{\frac{1}{5}}$ he tells us we may use $-x^5$ for P, as well as c^5, but that the first is preferable if x be very small (valde parvum), and the latter, if it be very great, when the truth is, that if x be ever so little greater than c, (and to make them equal would be absurd,) the series produced would not converge at all.

And hereupon I must crave thy pardon if I cannot admit of thy excuse for such mistakes, as if they were only set, as sums are to young people, for their practice in multiplying or dividing; for in that letter the author first very clearly and fully shewed his method of extracting, and freely communicated that most excellent and admirable invention of the uncial series, as I call

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it, but he appears to have designedly concealed, under pretence of avoiding tediousness, his method of applying fluxions to series, choosing only to give some illustrious examples of what might be performed by them. It therefore directly concerned his honour, that all he offered in that way should be perfectly well grounded and exactly true. Yet notwithstanding all this, though it shews that no one man can see every thing, and that Sir I. Newton could mistake, the world, as long as there remains in it any regard for science and sound knowledge, must ever revere that wonderful man's memory, and acknowledge him the greatest genius, in that way, that has ever been known to this day. But I must add, that though the knowledge of these methods is a pretty amusement, yet without a genius and extensive capacity, and particularly some knack at invention, they appear to me of but little use to be learned or studied. And thus far I have run on, chiefly to excuse the past trouble I have given thee: the appearance of a mistake in Sir Isaac Newton I thought was worth inquiring into, and by the favour of thy answer I am fully satisfied.

I have very little to say on the subject of instruments. But, as in thy teaching I formerly observed thy methods greatly excelled in neatness, so one instrument may for speed and certainty very much exceed another, and T. Godfrey's inventions, I think, were truly valuable. That by the reflecting speculums appears extremely so. I have here seen two of them as made by Hadley's direction, who enjoys both the reputation and profit of them, and I cannot but admire them. T. G. has indeed a fine natural genius for the mathematics, and it would, for the sake of his birthplace, the same with that of my own children here, be a great pleasure to me, if I could say that he wants no

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other interior qualification necessary in the composition of true merit. But now, though this letter may already be full long enough, I shall either for thine or my own diversion venture to eke it.

And as my charge to a grand jury last year, printed at their request, has fallen (I find) in thy way, I shall take the freedom further to observe to thee that the same subject, the noblest in my opinion that we can employ our thoughts and endeavours on, has been chiefly that of mine (while I had time to think) for above these two years past.

You had at the Royal Society my letter to P. Collinson on Generation, for which I have since received the President's thanks, in a very kind letter, in answer to one of mine on different subjects. That discourse, which you saw, was purely accidental; but the last winter but one I sent Peter, to whom, for his exact diligence in any thing I request of him, I am very much obliged, another discourse which I intended for the first chapter of a tract I then thought of writing, under the title of "The Duties of Man deduced from "Nature." That chapter is to prove that man, in his whole formation, was fitted and designed for society; in which I advanced a series of arguments, untouched by any author I had ever seen, previous to the others that are more common, which I did not omit. This has been well approved of by those who have seen it here, and on Peter's communicating it to thee, for which he has my leave, thou mayest not perhaps think it lost time to run it over and consider it, though being only a copy from my first draught, it would require a second hand to retouch it. The second chapter is on the Senses, which I believe would not prove more disagreeable, but this I would not venture to send till looked over again. The third

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is on the Intellect, treated in a different view from J. Locke's; but that subject, as I consider it, proves so exceeding nice, that it sticks as yet on hand. The fourth is on the Affections and Passions; this I caused to be transcribed, and sent the copy last summer to Peter, to be communicated to Dr. Mead, for his judgment on the anatomical part of it, having ventured to advance some things in that way, on the heart and nerves, that are somewhat and perhaps altogether new. I would also desire Sir Hans should see it, if his age admits him to be yet capable of such subjects; but Dr. Mead professing himself to be highly pleased with the performance, keeps it long in his hands, nor have I yet received his thoughts on it. In the next chapter on Moral Good or Virtue, where I state all the opinions I have met with concerning it and its foundation, and have, I think, reduced the whole to some certainty, I had advanced a good way, when by our late governor's death I had a troublesome charge thrown on me, that forces me on a different method of employing my thoughts. The whole therefore has been at a stand, and indeed I find the effects of years (as I am now near the end of my sixty-third) lie so heavy, after so busy a life, that I much question whether I must not drop the whole. The sixth chapter is on the Will. After which should follow an application of the whole, and the sum of it may in some measure be guessed at from the charge.

Thus I have run on to give thee a very long letter, yet hope it will not be found tedious. I forgot to mention in its proper place thy demonstration of J. Machin's proposition; for, to confess the truth, I had neglected it, as believing the thing itself not applicable to any real use. I thank thee however for thy trouble in it.

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I send thee the sheet L wanting in the Welsh Concordance: and finding there were more copies to be disposed of, I send three more complete ones in sheets, of which I desire thy acceptance, as they may serve for presents amongst thy friends in Wales, where they cannot doubtless but be accounted rarities, if nothing of that kind was ever done before in that language, though I know the Bible has had several additions in it. And in favour to the proprietors of the impression, who complain they are yet out of pocket by it, I shall add at their request, that they wish any person there would take off a parcel of them; but I know not how they would afford them, perhaps at about four shillings or four shillings and sixpence sterling, though they sell them considerably dearer here, printing being higher in this country than with you. But it is now full time to conclude; which I do with great respect and gratitude, from

thy much obliged and faithful friend,

J. LOGAN.

Philadelphia,
July 25, 1737.

CXVI.

G. ANDERSON TO JONES.

Sir,

Ever since I had the happiness of being known to you, I have ranked the occasion of it among the most fortunate incidents I have met with, and therefore would not omit any thing in my power to merit your regard. The pleasure I have in your company would excite me to make frequenter visits than might be agreeable to you, or indeed than the mutable situation

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I am in will allow; however, as our thoughts are conveyable in writing, letters, should they arrive out of season, may be laid aside till an opportunity offers, when their perusal will rather be an amusement than trouble. These were the reasons that induced me to take this method of sending you what follows, it being a new investigation of M. de Moivre's theorem at the bottom of page 247, Miscellanea Analytica, Prob. IV. Lib. VIII. viz. Si sol intra datum tempus tetigerit duos verticales hinc inde æqualiter ab ortu et occasu distantes, differentia earum altitudinum, quam habuit sub initio et fine temporis dati, major erit quam si duos quoslibet alios verticales intra idem tempus attigisset.

Your having the book by you will save me the trouble of repeating the problem; therefore I shall only observe that the above figure (Plate 3. fig. 5.) represents part of the sphere stereographically projected on the plane of the meridian, where Z is the zenith; C the elevated pole; MSDQ the parallel described by the sun the given day; S, Q, two places of the sun, so that ZQ, ZS, may be the sought verticals, and let Zsv be another vertical infinitely near the former ZS. Through S draw the almacantar vSR and the other lines as in the figure, where ZDO is the prime vertical.

Put l equal the sine of ZC, the comp. of lat.; $d=$ sine of CS the comp. of Decl.; also put $m=$ sine of the angle QZC; $M=$ sine of SZC; $a=$ to the altitude at Q; and A his altitude at S; $r=$ the radius. Then in the triangle ZCS we have $d:M::l:\frac{lM}{d}$ equal to the sine of the angle CSZ or sSv: and in the triangle Ssv, $r:\text{Ss}::\frac{lM}{d}:\frac{lM}{rd}\times\text{Ss}=\text{sv}$. Therefore $\frac{l\dot{M}}{rd}\times\text{Ss}=\text{sv}=\dot{A}$, but Ss is as the moment of its description and therefore given; whence Ss, l, r, d, are given quan-

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tities, therefore ($s\dot{v}$ or) $\dot{A}$, the augment of the altitude, is as M the sine of the corresponding azimuth, from whence the theorem is evident: for because $A-a$ by supposition is a maximum, therefore $\dot{A}-\dot{a}=0$ or $\dot{A}=\dot{a}$, but $\dot{A}$ is as M, and $\dot{a}$ as m, by what was said above, therefore $M=m$, hence the two sought verticals must be equally distant on each side the prime vertical, as Mr. de Moivre found. Q. O. E.

Coroll. Hence because the greatest sine is r, and the least o, therefore the sun increases his altitude fastest when he is on the prime vertical, and slowest when on the meridian.

Below is the solution to Simpson's problem in the Monthly Oracle for November last. I have not sent the investigation, it being only a simple corollary to the first case of the seventh prob. of the book of Quadratures.

The problem is this. Let A be the area of a curve, whose abscissa is z, ordinate $\overline{e+fz^n}|^m \times z^{p-1}$, 'tis required from thence to find S the exact area of another curve, whose ordinate is $\overline{e+fz^n}|^m \times z^{rn+p-1}$, r being any whole affirmative number. Put $t=p+\overline{m+1}.\,n$, $u=t+\overline{r-1}.\,n$, $w=p+\overline{r-1}.\,n$, and $R=\overline{e+fz^n}|^{m+1}$; then will

$$S=\pm\frac{e^r.\,w.\,\overline{w-n}.\,\overline{w-2n}.\,\overline{w-3n}.\,\text{\&c. to } p}{f^r.\,u.\,\overline{u-n}.\,\overline{u-2n}.\,\overline{u-3n}.\,\text{\&c. to } t}\times A$$

$$+\cdots\cdots\cdots\cdots\frac{1}{f.u}z^w.\quad R$$

$$-\cdots\cdots\cdots\cdots\frac{e.w}{f^2.u.\overline{u-n}}z^{w-n}.\quad R$$

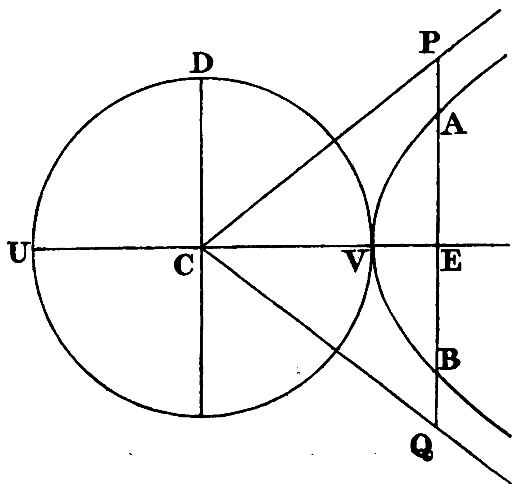
$$+ \cdots\cdots\cdots\cdots \frac{e^2.w.\overline{w-n}}{f^3.u.\overline{u-n}.\overline{u-2n}} z^{w-2n}.R$$

$$- \cdots\cdots\cdots\cdots \frac{e^3.w.\overline{w-n}.\overline{w-2n}}{f^4.u.\overline{u-n}.\overline{u-2n}.\overline{u-3n}} z^{w-3n}.R$$

$$\mp \frac{e^{r-1}.w.\overline{w-n}.\overline{w-2n}.\overline{w-3n}.\&\text{c. to } \overline{p+n}}{f^r.u.\overline{u-n}.\overline{u-2n}.\overline{n-3n}.\&\text{c. to } t} z^p \;.\; R$$

where we must observe, that the sign of the first term of this series will be plus when r is an even number, and minus when odd.

Mr. Simpson further adds, that from this series any arch of the Apollonian hyperbola may be found, and consequently the difference thereof and the corresponding asymptote (viz. PC − AV), which he requires to be found, when they are both produced to infinity.



Let AVB be the given hyperbola whose centre is C, transverse diameter UV = $2t$, asymptotes CP, CQ and on UV let the circle UDV be described; put $2p$ = para-

meter, and $r^2 = \frac{t+p}{t}$, then the difference sought will be expressed by the following series, viz.

$$\frac{1}{2r} + \frac{1.1}{2.4.r^2}A + \frac{3.3}{4.6.r^2}A' + \frac{5.5}{6.8.r^2}A'' + \&c. \times \text{UDV}^{a}.$$

Cor. Therefore if $p=0$, then the curve, asymptote, and axis coincide; in which case 'tis evident the difference will be $2\text{CV} = 2t$; but, because $r^2 = \frac{t+p}{t}$, if $p = 0$ then $r^2 = \frac{t}{t} = 1$, therefore $r = 1$, whence it follows that

$$\frac{1}{2} + \frac{1.1}{2.4}A + \frac{3.3}{4.6}A' + \frac{5.5}{6.8}A'' + \frac{7.7}{8.10}A'''\&c. = \frac{2t}{\text{UDV}^{a}} = \frac{t}{\text{DV}},$$

as observed by Mr. Stirling, Methodus Differentialis, page 59.

I am,
your most devoted and
most obedient servant,
G. Anderson.

Jan. 31, 1737-8.

N.B. In the above series A, A', A'', &c. denote the whole preceding term.

[a] The semicircle.

CXVII.

LOGAN TO JONES.

My esteemed friend,

Last year I returned thee my thanks most justly due for thy very obliging letter; and now send this, not to give thee, as my former did, any trouble; but rather for our mutual diversion, if it should happily

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yield thee any, as it gives me some in writing, though the subject I own is of no great importance; but the following is what at present offers.

It is now about two years since I almost resolved to look no further into any mathematical subject; yet it has once or twice happened otherwise. For this last winter making some inquiries into that of light, which I carried to some length, and by I know not what accident casting my eye on D. Gregory's problem, page 48 of his own first Latin edition of his Catopt. et Dioptr. Elementa, I could not think his solution just, and therefore I proposed the problem to myself in another manner, which having duly wrought out by fluxions, I had a biquadratic equation, for resolving whereof I had recourse to Dr. Halley's method, first published in the Transactions, and since subjoined to other treatises. But on proving his rules, which I examined with some care, I found two or three considerable mistakes in them, and thereupon began an attempt to set that most necessary part of algebra in a clearer light; since to form an equation, the prineipal business of that art, is of no use without knowing how to solve it when formed. But I found it was no proper time of life with me to attend such studies, and therefore dropped it.

But occasionally looking the other night into Du Hamel's History of the Academy of Sciences, (ad annum 1699,) on a very different view, I happened in his 558 p. to read these words: Doctor Ramondus Coninkius præpositus palatii Limæ Sacello libellum edidit, Limæ excusum anno 1696, in quo solutionem famosi problematis de cubi duplicatione a se inventam putat. Hunc librum misit ad Academiam D. Bruynsteem urbis Brugensis quæstor, atque ejus ea de re sententiam rogavit. D. de la Hire, cui id muneris datum est,

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ut problematis solutionem expenderet, paralogismum in ea delitescentem et satis involutum deprehendit. Now that so much notice should, so lately as in the year 1699, be taken of this problem, which consists only in finding two mean proportionals, I thought somewhat strange, and reflecting on it the next day, (yesterday,) a method for it occurred to me, so very simple and easy, that I could scarce think it possible but it must have been hit on by somebody long before. I hereupon examined what we have on the subject from Eutocius, in his Comment on Archimedes, L. 2. de Sph. et Cyl.; from Pappus's 3d and 4th books; from Eratosthenes, published in Greek by Bp. Fell, at the end of Aratus; also what Philander on Vitruvius has on the same, who is particular on it, page 178, &c. of Laet's edition; but I could nowhere find any thing resembling mine. And the excellent Dr. Barrow, in his Compend. edit. of Archimedes, [p. 31.] on the place before quoted, having these words, Hoc problema solidum est, ad ipsius quippe solutionem exigens, ut duæ mediæ proportionales inveniantur, quod præstare nequit geometria communis, regula tantum utens et circino: per conicas sectiones, et aliis compluribus modis effici potest de quibus hic taceo: I from thence concluded that, in his time at least, there was no plain and simple method known for it. Therefore as I take mine to be truly such, and the problem in itself is allowed to be a noble one, if there are any such left, as I hope there are divers, who have yet a regard for geometrical effection or construction, and depend not wholly on logarithms and series, such a solution may be acceptable to them, if not known before. If it be, it is but little labour lost, and I have my invention only to myself; if otherwise, I leave it wholly to thy discretion and judgment to communicate, or, if proper, to publish it, in which

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view I have drawn it up in Latin, as I find most things of the kind are in the Transactions, for the benefit of foreigners as well as our own people.

When I began, this I expected more time, but now find myself reduced to nearly the last moment I can have by this opportunity, and therefore must abruptly close, which I do, with sincere respect and gratitude, from

thy affectionate and obliged friend,

J. LOGAN.

Stenton, in Pensilvania,
March 31, 1738.

Quantum se torserint veteres de problemate Deliaco (ut olim nuncupatum est) expediendo, seu cubi duplicatione, i. e. duobus mediis proportionalibus inveniendis, ex Eutocio, Eratosthene, Pappo et aliis satis notum est. Huic enim unico problemati deberi fertur conchoidis et cissoidis inventio, quinimo primos in conicis progressus hinc ortum duxisse creditur. Solutiones ejus diversimodæ repertæ sunt, at perplexæ vel operosæ fere omnes, ut ex dictis autoribus abunde patet. Quid melius et paratius ad effectionem geometricam, (nam de numeris constat,) a nuperioribus præstitum sit nescio. Mihi vero occurrit solvendi methodus, quæ si nondum aliis innotuerit, haud indigna fortasse videbitur quæ publici fiat juris.

Problema solidum esse, et simplici geometria, seu recta linea et circulo solvi non posse, a geometris perhibetur. At si sequens methodus, sola rectarum ope, perinde facilis comperiatur atque Euclideorum problematum pæne dixerim facillimi solutio, quidni pro pure geometrica haberi poterit vix constare videbitur. Ea autem hæc est.

Datæ duæ lineæ AB et BC ad angulum rectum in B

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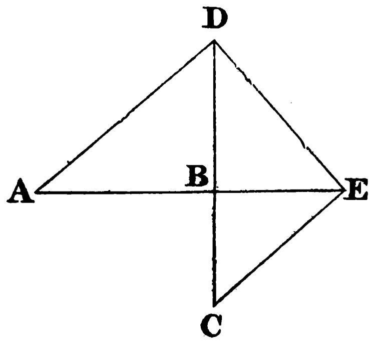
componantur, producantur autem versus E et D; ducantur porro duæ parallelæ per data puncta A et C, secantes alteras productas in D et E, ita vero, ut linea DE has parallelas connectens sit utrique perpendicularis; et factum est quod quæritur, ut ex



ipso intuitu patet. Triangula enim CBE, EBD et DBA, propter angulos rectos sunt similia, ideoque ut data BC ad BE, ita BE ad BD, et ita BD ad datam AB, et inter datas duæ sunt mediæ BE et BD. Q. E. I.

Regeretur fortasse effectionem non pure geometricam esse; non directe enim dari puncta E et D, sed experiendo tantum reperiri. Sed cum norma aut quadra instrumenta sunt æque ac circinus geometriæ simplici inservientia, hujus ope res facillime effecta dabitur: promptissime, vero, si adhibeatur scala illa apertilis seu diductilis, quæ in thecis organorum geometricorum ad ducendas parallelas plerumque habetur. Si alterum latus enim ad punctum A applicetur, et alterum ad C, duo autem anguli ejus ad rectas BD et BE pariter applicentur, quod factu est facillimum, expeditissime definientur hæc puncta. Atque hoc pacto duo media geometrica, haud operosius quam unicum ex Euclidis præcepto, dantur.

I. L.

CXVIII.

LOGAN TO JONES.

My esteemed friend,

This comes solely to crave thy excuse for my last, wherein I ventured on a solution of the Deliac pro-

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blem: and though what I gave will, I imagine, be found the readiest and best method of any that have been applied for geometrical construction, yet having since discovered what from my first thought of it I suspected, that it is not new, I must own myself much too rash in sending it away: but, an opportunity just then presenting, I unhappily made use of it, and the article, in what I said both in my letter and the other paper, that gives me the most concern, is my mentioning those ancients, Eratosthenes, Pappus, and Eutocius, as having left us no such method as[b] mine in their writings, which I now find is a mistake, and was thus occasioned.

Eratosthenes and Pappus I have, and thought I also had in Rivaltus' (alias Flurantius's) edition of Archimedes, in folio, Paris 1615, all that Eutocius had left in his comment on that great author; but looking some time after into G. Vossius de Scient. Math., I there found he had in that comment given the several methods of twelve ancient mathematicians, all named there [p. 72.] by Vossius. Hereupon recollecting that I had seen, in this country, amongst some books that had belonged to a learned German, who many years since was coming over hither, but died in the way, a translation of Archimedes's works into high Dutch by Sturmius, in a pretty large folio, I procured it; and there found all those several methods described at large, the very first of which, being Plato's, proves exactly the same with mine. But as he directed the making of an instrument for the purpose[c], of which the figure is

[b] See the Commentary of Eutocius on the second Prop. of the second Book of Archimedes on the Sphere and Cylinder.

[c] Plato's method consisted in taking two right angled triangles, which had one side common to both of them, while their hypotenuses cut each other at right angles. The machine which he devised for finding such figures may be seen at p. 135 of Torelli's Archimedes: it consisted of a straight base, on which were

there given, it appears the more mechanical; yet in effect that instrument and the other I mentioned, which we have in our instrument-cases for drawing parallel lines, are very much the same. Thus by that defect in Rivaltus, of which I was not then apprehensive, (for I never yet could meet with Archimedes with Eutocius on him, either in the Greek edition of Basil, or of Commandinus's Latin version,) I was led into the mistake. And I now earnestly request that if any other person or persons of skill in these things, besides thyself, should have seen my letter or inclosed paper before this comes to hand, thou wouldst be pleased also to shew him this, for removing the imputation of a presumption, of which I would by no means be guilty, and therefore not be thought so.

But having further mentioned Dr. Halley's rules for resolving affected equations, first printed in the Philosophical Transactions[d], and several times since in other books, as if there were some errors in them; lest I should be thought to mistake in this also, I shall here, from my notes when on the subject, point out some, though very briefly, referring to the tract itself to render what I say more intelligible.

For the unknown letter or root he substitutes, as is done in all the methods directed for the same purpose, a binomial $a \pm e$, and valuing a by a number, the nearest that can be guessed at for the root, with that number + or − e, he brings out another equation, which gives the value of e only, and to find this from the first three terms of it, viz. the absolute number, for which he puts b, and e and ee with their coeffi-

placed two pieces at right angles to it, and in the same plane with one another. These were furnished with grooves, in which the ends of a moveable rod were confined, so as to keep it always parallel to the base.

[d] Vol. XVIII. p. 136.

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cients, expressed by s and t, that is from $\pm b$, $\pm se$, $\pm tee$, he forms a quadratic equation, and solves it in the common way, by completing the square, though he explains not his process to his readers.

But owning there is some difficulty in judging whether e be $+$ or $-$, he gives these two rules: that the signs of b and s must and will always be contrary, and that if we have $+b$, we must put $-se$, and therefore 'tis $a-e$ or a is taken too great, and on the contrary $-b$ gives $+e$, and then a is taken less than just.

But these rules, especially the latter, do not at all hold, and one cannot sufficiently admire, that so great a master in those sciences, as that gentleman is known to be, should so widely mistake in a point of the greatest moment in his rules, and which he might easily have seen into with a little application. For the sign of b, $+$ or $-$, depends not so much on taking a too great or too little, as on the proportion of the coefficients to each other, in their respective places, which ought naturally to bear some proportion to a gradation of the roots ascending, which will best appear from an example.

The fourth power of $x-3=0$ is $x^4-12x^3+54x^2-108x+81=0$. Let some of these coefficients be varied, still keeping up the same value;

$$\text{(A)}\quad x^4-8x^3+54x^2-144x+81=0$$
$$\text{(B)}\quad x^4-16x^3+54x^2-72x+81=0$$

or all of them thus;

$$\text{(C)}\quad x^4-10x^3+51x^2-126x+108=0$$
$$\text{(D)}\quad x^4-14x^3+58x^2-90x+45=0$$

Then if we try all these by $2+e=x$, we shall have these remainders:

$$\begin{array}{llllll} & & b & s & t & u \\ \text{from} & \text{(A)} & -39 & +8e & +30e^2 & * +e^4 \\ & \text{(B)} & +41 & -16e & -18e^2 & -8e^3+e^4 \\ & \text{(C)} & -4 & -10e & +15e^2 & -2e^3+e^4 \\ & \text{(D)} & +1 & +6e & -2e^2 & -6e^3+e^4 \end{array}$$

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Again, if we try them by $4-e=x$, we shall have these:

$$\begin{array}{lll} \text{from} & (A) & +\overset{b}{113}-\overset{s}{160}e+\overset{t}{54}e^2-\overset{u}{8}e^3+e^4 \\ & (B) & -111+152e-42e^2 \quad * \quad +e^4 \\ & (C) & +\ 36\ -\ 58\,e+27e^2-6e^3+e^4 \\ & (D) & -\ 27\ +\ 42\,e-14e^2-2e^3+e^4 \end{array}$$

Now as all these in the first range downwards arise from $a+e$, and all in the second from $a-e$, yet we have $+b$ and $-b$ indifferently in both, we see his principal rule does not at all hold. And in (C) and (D) of the first range, we have both $-b-se$, and $+b+se$, which also contradicts the other.

These examples might be sufficient to prove what I have said; yet I shall further observe, that when an equation has more real or affirmative roots, to say nothing of others, these rules must often necessarily fail; for instance, this equation $x^4-38x^3+529x^2-3192x+7056=0$ has these two roots $x=7$ and $x=12$, and, if tried by 10, it gives the remainder $+36\pm12e-11e^2\pm e^3+e^4$: it gives also the very same if tried by 9; and whether we take $+e$ or $-e$, b is always $+36$, and the value of e is $=+2$, -2, $+3$, and -3, that the numbers may answer both roots, as $9+3=12$, $10-3=7$; $10+2=12$ and $9-2=7$.

So this, $x^4-140x^3+7324x^2-169680x+1468800=0$, has these four roots 30, 34, 36, 40, and if tried by 35, it gives the remainder $\overset{b}{25} \; * \; -\overset{t}{26}e^2 \; * \; +e^4$, in which $e=+1$, -1, $+5$, and -5; and if tried by 32 and by 38, it gives the same for both, viz. $-128\pm48e+28e^2\pm12e^3+e^4$, in which e is $=+2$, -2, $+4$, -4, $+8$, and -8, that it may equally answer all the several roots; a speculation truly delightful to see, how admirably numbers thus qualified are formed.

Further again, if a true canonical equation or power be tried by any other numbers equally distant from its

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just root, the remainder for each will be the same: as if $x^4-24x^3+216x^2-864x+1296=0$, the fourth power of $x-6$, be tried by 4 and by 8, each of them will give the same, viz. $+16-32e+24e^2-8e^3+e^4$. But is it not strange, that if in that equation instead of $-24x^3$ we put $-25x^3$, the root will sink from 6 to less than 3,5, and if we put $-23x^3$, it becomes, if I mistake not, impossible?

I shall yet add, that as all quadratics cannot be solved by completing the square, so it happens here that when the signs of *b* and *t* are alike, (the meaning of which is, when to form the equation, we must have $-b$, and the Doctor should have so explained it,) if *b* is greater than $\frac{1}{4}ss$, the solution, by this method, becomes impossible, as in the preceding second (C) where $36 \times 27 > (29)^2$, or which is the same $\frac{36}{27}$ $(=1\frac{1}{3}) > (\frac{29}{27})^2$; and so in many others. I am sensible, however, that in many cases this is an excellent method; yet there are some others that I would generally prefer.

I could add some other remarks, but even this from me may justly require an apology; for I own it is trifling away my time. But since I have for some years past been confined to crutches, amusements are necessary, and I fell into this solely by resolving one biquadratic equation (as I said) on the subject of light, which I was then closely considering. These remarks, however, may be the better excused, since the resolution of affected equations is an article of as great importance, as any I know, in algebra; for it can be of no use to know how to bring a question to an equation, unless we can resolve it when formed.

I am, with sincere respect,

thy obliged and affectionate friend,

J. LOGAN.

Philadelphia, 4th of May,
1738.

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I hope the three Concordances, with my letter, sent thee last year, were received. And, if I have made any mistake in the preceding numbers, be pleased to excuse and rectify them, if worth so much of thy notice.

J. L.

P.S. Since I wrote the enclosed[e], and just as I was about making it up, I have thought of a method of exceedingly improving the Doctor's rational one, conformably with one I had hit on for myself, which is never to renew the work from the first given equation, but to proceed solely on the first remainder, by first finding the value of *e* for one or two figures, and then for the second, &c. to take $e\ (second) = \frac{b}{s \pm t \times e\,(first)}$ and $e\ (third) = \frac{b}{s \pm t \times e\ (second)}$ &c. which I am of opinion will be found the easiest and best method that has been yet published, or that I have seen, at least as far as I can remember. Yet if we make it $e\ (second) = \frac{b}{s \pm te \pm uee\ (first)}$ it will be considerably better.

J. L.

CXIX.

BOUGUER AND LA CONDAMINE TO HALLEY.

Monsieur, Quito, Mai, 1738.

Quoique nous n'ayons pas partagé avec M. Godin l'avantage que lui a procuré son voyage d'Angleterre, de vous connoître personnellement, nous ne nous croyons

[e] The postscript is written on the cover in which the letter was enclosed.

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pas dispensés de vous communiquer le résultat de nos travaux sur l'obliquité de l'écliptique, que nous avons toujours regardé comme un des principaux et des plus utiles objets de notre voyage sous l'équateur. Nous n'avons pas vu l'écrit de M. Godin depuis les derniers changemens qu'il y a faits; nous savons seulement que, d'accord sur le plus grand nombre des observations, nous ne laissons pas d'être un peu différens sur les conséqnences, et nous ne pouvons trouver de juge plus éclairé, ni plus désintéressé, qu'un savant que toutes les nations s'accordent à reconnoître pour le premier astronome de l'Europe. Nous joignons à cette lettre nos mémoires, qui contiennent l'histoire de nos observations communes et les conclusions que chacun de nous en a tiré. Il nous reste encore à éxaminer les divisions de l'instrument. Nous avons remis à nous satisfaire entièrement sur cet examen, que nous n'avons que commencé, au tems où nous observerons avec le même instrument la différence en latitude aux deux extrémités de l'arc du méridien, que nous sommes actuellement occupés à mesurer. Mais nous doutons que cela puisse apporter trois ou quatre secondes de changement à notre détermination.

Nous ne nous flattons pas, Monsieur, de pouvoir recevoir de réponse à cette lettre en ce pays-ci; mais nous espérons la trouver à notre retour en France. Elle pourra nous être remise par M. de Fontenelle, ou par M. de Maupertuis qui doit être de retour de son voyage du nord, où lui et les autres mathématiciens, nos confrères, n'ont eu à vaincre que les difficultés du froid du climat, qui nous ont été communes avec eux contre toute apparence, mais où ils n'ont pas sans doute trouvé les obstacles que l'ignorance, les préjugés, la barbarie et l'impossibilité de faire éxécuter les ordres du souverain nous suscitent ici à chaque pas.

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Nous avons l'honneur d'être avec la plus haute estime et la vénération due aux grands hommes.

Monsieur,

vos très-humbles et très-obéissants serviteurs,

BOUGUER. LA CONDAMINE.

There is, in Lord Macclesfield's collection, a manuscript entitled "Relation des Observations, faites à Quito, de l'obliquité de l'Écliptique au dernier solstice de 1738, et au premier de 1737, avec un instrument de douze pieds de rayon par M. Bouguer." There is likewise a similar memoir of La Condamine, between the leaves of which the above letter was found. They were both translated into English and printed at London, as La Condamine says, "sans que nous en ayons eu connoissance," (Journal du voyage fait par ordre du Roi à l'équateur, p. 33.) La Lande says, "Ces observations furent supprimées quoique La Condamine les ait toujours défendues comme bonnes." Bibliographie Ast. p. 410.

CXX.

LOGAN TO JONES.

Stenton in Pensilvania, 16th Oct. 1738.

Esteemed friend,

I should not now take notice of my letter of last year being unanswered, but that I know not to this minute, whether the three Welsh Concordances that accompanied it were received. My next to it, of the 31st of March last, was unhappily so far out of the way, that I was obliged to give thee the trouble of another to apologize for it. This now comes on what probably may be allowed a more justifiable occasion, which is this.

After I was, in June last, happily released from a charge, that, by the unkindness of a neighbouring government, had given me no small uneasiness, looking again into the Philosophical Transactions, No. 440, in

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which something of mine had been most erroneously printed, making me, at the end of page 194[f], and beginning of the next, speak utter nonsense, I cast my eye on the next preceding article but one, which is a proposition by J. Hadley relating to the combination of transparent lenses with reflecting planes, which affording me some matter for reflection, I from thence turned to Hugens's Dioptrics there quoted, (a work I had by me many years, yet till then had never read three pages in it,) and in viewing his twelfth proposition besides some others, I was shocked, I confess, at the tediousness as well as the obscurity of the demonstrations, (if they are to be called such,) that such admirable rules were attended with. Being therefore then at leisure, I resolved to try whether by some means or other I could not hit on some plainer, and succeeded in it, as I have said in the introduction of the enclosed[g], so much to my satisfaction, both in the analytic and geometric way, and so far exceeding any thing I could meet with in the authors I have on the subject, that thinking it a pity the discoveries should die in my hands and be lost, I resolved to commit both methods to writing, which I did in English; but observing it was somewhat long, that Hugenius's work was in Latin, and that he himself had always in his writings preferred the methods of the ancients in demonstrating by geometrical construction, I chose to run this over again in that language, which is not unfamiliar to me, and send it thee in the manner it

[f] In justice to the editor of the Philosophical Transactions it must be stated, that Mr. Logan, probably from imperfect recollection, has here committed a mistake. His own paper is preserved by the Royal Society, and the printed text only varies from it, in this part, at the third line of p. 195, where, to avoid a repetition of the word, "their" has been altered to "the": and this no way affects the meaning of the passage.

[g] Nothing of this kind was found enclosed in the letter.

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appears in the enclosed Latin copy, done by a pretty exact hand. Whether the Society may think fit to publish it in the Transactions[g], (for I have no way of preserving it but in some such collections, at home or abroad,) is left to your own judgment to determine. I need say nothing to recommend it; for the justness of the construction, the clearness of the demonstration, and the beautiful uniformity of the schemes, as applied to the several differences in the various cases, will do it abundantly to any mathematical reader; all which is owing to the subject, not to me, for I claim no merit from it, being conscious of having no other than the chance of an inventor, and the labour of digesting. But as I had drawn up both parts, the analysis and construction, as I have said, together in one, and afterwards chose to throw out the first, as judging the latter preferable, yet that first, if I mistake not, may upon examination be found worth preserving, and therefore I have got another hand to copy that also, which I here likewise enclose with the other, for thy own use, or to be applied as thou shalt judge proper, for I commit that wholly to thy disposal.

When I had gone through these, I proceeded to consider also the aberration of rays from the right focus, when they fall on the glass at a distance from the vertex, which Hugens has laboriously calculated; and Sir Isaac Newton in his Optic Lectures, p. 132, has also a proposition for the same, which he solves by an intricate infinite series. For this also I happened to light on a simple and clear method. And as that of Hugens answers (I think) no further than the sines and their versed sines hold in a duplicate ratio, and

[g] No such paper by Logan appears to have been inserted in the Philosophical Transactions. This may be accounted for by what is said in a subsequent part of the letter.

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neither of them for other than parallel rays, mine extends to all cases of oblique as well as parallel rays, though not with equal simplicity, and to all distances from the vertex, expressing in them all the quantity of the aberration in the terms of the versed sines only; so that from these processes, I believe, a good account may be given of the whole business of refraction as applied to that useful and entertaining science of dioptrics.

What I here now give, or have mentioned, may probably appear somewhat strange to thee from me, after my former honest declarations of the shortness of my skill in these sciences; but what I then professed I do sincerely still; for when I look into the works of divers others, I cannot esteem myself other than a smatterer in them, and but a mean one. The only advantage I have is a disposition to see, as far as my optics will carry me, into my subject, which, with application when I can use it, has sometimes helped me to a tolerable view of what I was in quest of. But little it is we do or can know here. In nature, scarce any thing beyond what, in Pliny's sense, may be called the history of it; in morals we may much more, as these are ordained for our grand duty in life, and powerful lights are breaking out on that quarter; but for bare speculation only, number and measure appear to me to be the most adequate objects on earth for the human intellect, (though this is infinitely short of being adequate to the extent of those,) but in the operations on the ideas of these there is certainty, to which in most other points (for faith is not knowledge), moral excepted, we are to remain, I doubt, for ever strangers. For this reason it is that, after a life of business, now at full sixty-four, though within these two years I intended the contrary, I can sometimes find as agreeable an amusement, not in reading but in thinking on these

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subjects, as in any that occur to me, and what I here give are some of the fruits, which had their first rise from the accidental occasion I have mentioned.

I shall add that, if this be published, I desire it may be done with due care and with justice to me, for I was ill used in the other. Nor can I think it was quite right, though there was no injustice in it, to publish[h] only that part of my letter to your president, about the angular appearance of the darts of lightning, which gave barely the thought, unattended, and consequently unsupported, by that clear instance I gave of the very same appearance in a straight rod or line, viewed at some little distance through the wavy glass of a window, which puts it in every one's power to judge of the probability of the solution, when they might not otherwise see into the reasonableness of it.

I designed to have sent thee my whole English tract, but the copyist was obliged to leave it unfinished. He had, however, gone through all the analytic part, and entered on the geometric; therefore cutting[i] off what little he had done in this, because imperfect, I send only that first, which is all that can be necessary, since you have the other fully in the Latin.

If the Society think fit to publish the Latin one, I request it may be speedily resolved, and if declined, that the piece, the Latin one I mean, after thy perusal of it, may be delivered to my friend P. Collinson, who, at my desire, will forward it to a friend in Holland, where I know they will be glad to publish it, and perhaps that may be the most proper place: since Hugens's works were printed there. But I should rather choose to see it first appear in England. The reason of my mentioning Holland is, because I have

[h] Philosophical Transactions, vol. XXXIX. p. 240.
[i] Logan has written in the margin, "It is not cut off."

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been much solicited, by some of the most learned there, to suffer some other Latin pieces, they have seen of mine, to be printed amongst them.

I know not whether it may be to any purpose to request any kind of answer from thee. I am, however, for thy one past favour,

thy much obliged friend,

J. LOGAN.

P. S. If the Society should incline to publish this Latin piece, but think it refers too particularly to a printed book, when it should rather mention the rules only, and give the demonstrations of them, I shall not be against an alteration, if made by a good hand, skilful both in the language and science, and with judgment, provided that the rules be mentioned as Hugens's, and nothing of mine be left out besides those references.

I was in hopes to have got the other smaller part or appendix, demonstrating the aberrations, ready for this opportunity, but could not. I propose, however, to send it this fall; but this need not retard the publication of the other; for in my opinion they would stand altogether as well in two several Transactions as in the same. If any errors appear either in the figures or writing, that have been overlooked, pray please to correct them.

(If to be favoured with an answer) I beg to be informed in it with what view the Society caused that proposition of J. Hadley, which I have in my letter modestly said afforded me some matter of reflection, to be published in their Transactions. For it is beyond my reach, I own, to find out the design of it. There is a star, placed in the figure, sending its rays to the speculum, which surely will be allowed, if any, to come parallel; and it is well known a speculum must reflect

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them equally parallel: and surely no mortal will ever allow that a telescope is to be so placed as to receive parallel rays otherwise than as parallel as may be to the axis. What then can be meant by those angles? Pray condescend so far as to inform the ignorant, for I am entirely so.

J. L.

CXXI.

MACHIN TO GAEL MORRIS.

Dear Sir, Thursday, May 3, 1739.

Dr. Halley comes sometimes to Batson's of a Thursday. If this be his day, and you happen to see him there, be so good as to ask him whether Dr. Bevis gave him a paper of mine last Saturday. If he says he did, then pray tell him that I forgot to send the constant numbers to be applied to the comet of 1531, which I understand he is now examining, and communicate them to him as underneath. But if Dr. Bevis has not seen him since, or let him have that paper, then you need not shew him any thing here, for he will not know what I mean. I would come myself, but cannot get my business ready time enough.

I am your most obliged

humble servant,

J. Machin.

		Logar.	
$p=$	0.0326085	8.5133308	$\overline{nnt-nn-1}=9.735813$
$nn=$	10.030277	1.0013128	
$n=$	–	0.5006564	
$T=$	–	9.3126579	
$\frac{1}{3}nR=$		1.7816577	the limit, $\frac{2Rp}{t}\sqrt{P}$, is
$\frac{3T}{nR}$		7,5310002	about $\frac{1}{19}$
$P=$		7.8259887	

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CXXII.

G. ANDERSON TO JONES.

Sir,

I have sent you the investigation of my theorem for approaching the roots of affected equations universally, which you'll find nearer than either the rational or irrational theorem of Dr. Halley. 'Tis true the expression is somewhat more complex, but often thereby the root will be determined sufficiently exact without renewing the operation, as must be done in using those theorems of the Doctor. Also the method, taken in its invention, may frequently be usefully applied in the solutions of such problems, where an approximation is all that can be had; one instance of which I shall give in finding the root of the exponential equation $x^x = d$.

Let $Ax^m + Bx^{m-1} + Cx^{m-2} + Dx^{m-3} + Ex^{m-4} + Fx^{m-5} + \&c. = 0$ be any affected equation, where A, B, C, D, &c. are given quantities, indifferently affected with the signs $+ -$: and let a be taken as near x as possible.

Then $x = a - \dfrac{2pa}{p + q - \dfrac{rp}{q}}$ quam proxime.

The values of p, q, and r being as here under:

$$p = \begin{cases} +Aa^m \\ +Ba^{m-1} \\ +Ca^{m-2} \\ +Da^{m-3} \\ +Ea^{m-4} \\ +Fa^{m-5} \\ +\&c. \end{cases} \qquad q = \begin{cases} +\overline{m-\ \,0}.\,Aa^m \\ +\overline{m-\ \,2}.\,Ba^{m-1} \\ +\overline{m-\ \,4}.\,Ca^{m-2} \\ +\overline{m-\ \,6}.\,Da^{m-3} \\ +\overline{m-\ \,8}.\,Ea^{m-4} \\ +\overline{m-10}.\,Fa^{m-5} \\ +\&c. \end{cases}$$

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$$r = \begin{cases} +\dfrac{m-0}{1}.\dfrac{m-1}{2}. & Aa^m \\ +\dfrac{m-1}{1}.\dfrac{m-4}{2}. & Ba^{m-1} \\ +\dfrac{m-2}{1}.\dfrac{m-7}{2}.+1. & Ca^{m-2} \\ +\dfrac{m-3}{1}.\dfrac{m-10}{2}.+3. & Da^{m-3} \\ +\dfrac{m-4}{1}.\dfrac{m-13}{2}.+6. & Ea^{m-4} \\ +\dfrac{m-5}{1}.\dfrac{m-16}{2}.+10. & Fa^{m-5} \\ +\&c. & \end{cases}$$

By supposition a is nearly equal to x, therefore y being put equal to $\dfrac{x-a}{x+a}$, it will be a very small quantity, whence y^3, y^4, y^5, &c. will be comparatively nothing with respect to y, y^2; therefore all powers of y above y^2 may be neglected or put equal to 0, but since $y=\dfrac{x-a}{x+a}$ we have $x=\dfrac{1+y}{1-y}\times a$, therefore for the several powers of x in the given equation substitute the corresponding powers of $\dfrac{1+y}{1-y}\times a$, and we have

$$Ax^m = \frac{\overline{1+y}|^m}{\overline{1-y}|^m} \times Aa^m = \frac{Aa^m \times \overline{1+y}|^m \times \overline{1-y}|^0}{\overline{1-y}|^m}$$

$$Bx^{m-1} = \frac{\overline{1+y}|^{m-1}}{\overline{1-y}|^{m-1}} \times Ba^{m-1} = \frac{Ba^{m-1} \times \overline{1+y}|^{m-1} \times \overline{1-y}|^1}{\overline{1-y}|^m}$$

$$Cx^{m-2} = \frac{\overline{1+y}|^{m-2}}{\overline{1-y}|^{n-2}} \times Ca^{m-2} = \frac{Ca^{m-2} \times \overline{1+y}|^{m-2} \times \overline{1-y}|^2}{\overline{1-y}|^m}$$

$$Dx^{m-3} = \frac{\overline{1+y}|^{m-3}}{\overline{1-y}|^{m-3}} \times Da^{m-3} = \frac{Da^{m-3} \times \overline{1+y}|^{m-3} \times \overline{1-y}|^3}{\overline{1-y}|^m}$$

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$$Ex^{m-4} = \frac{\overline{1+y}\,\big|^{m-4}}{\overline{1-y}\,\big|^{m-4}} \times Ea^{m-4} = \frac{Ea^{m-4} \times \overline{1+y}\,\big|^{m-4} \times \overline{1-y}\,\big|^{4}}{\overline{1-y}\,\big|^{m}}$$

$$Fx^{m-5} = \frac{\overline{1+y}\,\big|^{m-5}}{\overline{1-y}\,\big|^{m-5}} \times Fa^{m-5} = \frac{Fa^{m-5} \times \overline{1+y}\,\big|^{m-5} \times \overline{1-y}\,\big|^{5}}{\overline{1-y}\,\big|^{m}}$$

&c. &c. &c.

Therefore,

$$\left.\begin{array}{l}
\left\{\begin{array}{l} Ax^m = Aa^m + \overline{m-0}\,.Aa^m\,y + \\ \frac{m-0}{1}.\frac{m-1}{2}.\quad Aa^m\,y^2 \text{ &c.}\end{array}\right. \\
\left\{\begin{array}{l} Bx^{m-1} = Ba^{m-1} + \overline{m-2}\,.Ba^{m-1}y + \\ \overline{\frac{m-1}{1}.\frac{m-4}{2}.} + 0 \times Ba^{m-1}y^2 \text{ &c.}\end{array}\right. \\
\left\{\begin{array}{l} Cx^{m-2} = Ca^{m-2} + \overline{m-4}\,.Ca^{m-2}y + \\ \overline{\frac{m-2}{1}.\frac{m-7}{2}} + 1 \times Ca^{m-2}y^2 \text{ &c.}\end{array}\right. \\
\left\{\begin{array}{l} Dx^{m-3} = Da^{m-3} + \overline{m-6}.Da^{m-3}y + \\ \overline{\frac{m-3}{1}.\frac{m-10}{2}} + 3 \times Da^{m-3}y^2 \text{ &c.}\end{array}\right. \\
\left\{\begin{array}{l} Ex^{m-4} = Ea^{m-4} + \overline{m-8}\,.Ea^{m-4}y + \\ \overline{\frac{m-4}{1}.\frac{m-13}{2}} + 6 \times Ea^{m-4}y^2 \text{ &c.}\end{array}\right. \\
\left\{\begin{array}{l} Fx^{m-5} = Fa^{m-5} + \overline{m-10}.Fa^{m-5}y + \\ \overline{\frac{m-5}{1}.\frac{m-16}{2}} + 10 \times Fa^{m-5}y^2 \text{ &c.}\end{array}\right.
\end{array}\right\} = 0$$

That is, from the substitution on the other side of $p+qy+ry^2=0$; but because y is much greater than y^2, $y = -\frac{p}{q}$ nearly; but $y = -\frac{p}{q+ry}$, therefore $y = -\frac{p}{q-\frac{rp}{q}} = \frac{x-a}{x+a}$, whence $x = a - \frac{2pa}{p+q-\frac{rp}{q}}$. Q. O. E.

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Again, let $x^x = d$, and take a as near x as possible; put $L = \log. d$, $l = \log. a$ and n equal to the subtangent of the logarithmic curve, then y being taken equal to $\frac{x-a}{x+a}$, by the common methods the logarithm of $\frac{x}{a}$ is $2n \times \overline{y + \frac{1}{3}y^3 + \frac{1}{5}y^5 + \&c.}$; but y being a very small quantity by supposition, the log. of $\frac{x}{a}$ is nearly $2ny$, hence $2ny + l = \log. x$; this being multiplied by x is the log. of x^x; hence the following equation, $\overline{2ny + l} \times x = L$; that is, $\overline{2ny + l} \times \frac{1+y}{1-y} \times a = L$,

$$\text{or } \left.\begin{matrix} 2any^2 + 2na \\ +\ la \\ +\ L \end{matrix}\right\} y = L - la.$$

In the last equation neglect the term $2nay^2$ as being very small in respect of the others, then $y = \frac{L - la}{2na + la + L} = \frac{x-a}{x+a}$, whence $x = a + \frac{\log. \frac{x^x}{a^a}}{n+l} = a + \frac{L - la}{n+l}$, or nearer,

$$x = a + \frac{2a \times \overline{L - la}}{\overline{l+n} \times 2a + \frac{L - la}{2na + la + L} \times 2na}.$$

But if in the equation $\overline{2ny + l} \times x = L$, for y its value be put, then $\overline{2n \times \frac{x-a}{x+a} + l} \times x = L$, and from hence $x = p \pm \sqrt{p^2 + qa}$, where $2p = \frac{L + \overline{2n - l} \times a}{2n + l}$, and $q = \frac{L}{2n + l}$. Any of these theorems approximates exceeding fast, so that from hence it is easier to find x in this equation $x^x = d$, than in this $x^3 = d$.

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I should have sent an example of each of these theorems in numbers, but my papers are in London, and I have ne'er a table of logarithms by me. I shall be in London about Whitsuntide, when I propose myself the pleasure of seeing you; in the meantime I remain, with great respect,

your most obedient and most humble servant,

G. ANDERSON.

From Hothfield in Kent,
May the 10th, 1739.

CXXIII.

G. ANDERSON TO JONES.

Sir,

I was favoured with yours of the 11th, but not soon enough to answer it by the return of the post. The globes came safe to hand this day sennight, which I like very well, but have not Bayer's Catalogue of the Stars to make any use of the notes which are thereon. I hope by the next post Mr. Senex will have his money, there being none more exact in things of that kind than my Lord: the reason why an order was not sent sooner, was occasioned by having omitted sending Mr. Senex's Christian name, that so he might draw upon Mr. Hoare in form; but whether Mr. Senex will have notice of such an order I know not, though guess some of my Lord's people will be directed to write to him for that purpose; but if not, he may send to Mr. Hoare's about Thursday or Friday next, and I am certain he will receive his money; therefore hope you will have no further trouble, I being quite ashamed to have given you so much already.

I can't see how the sines I sent you for binomial

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curves can be easily deduced from the fifth prop. of the quadratures, for in that $z^{\theta} R^{\lambda}$ is to be multiplied into a series of this form $A + Bx^{\eta} + Cx^{2\eta} + Dx^{3\eta}$ &c. whereas in mine $z^{\theta} R^{\lambda}$ is to be multiplied into one of this form, $A + Bx^{\eta}R^{-1} + Cx^{2\eta}R^{-2} + Dx^{3\eta}R^{-3}$ &c. I shall send you my method of investigation, which is a particular artifice I learned from De Moivre: put v for the area of the curve whose ordinate is $x^{\theta-1} \times \overline{e+fx^{\eta}}|^{\lambda}$, R being equal to $e+fx^{\eta}$, and therefore $\dot{R} = \eta f x^{\eta-1}\dot{x}$: hence from the prin. of quadrat. $\dot{v} = x^{\theta-1}\dot{x}R^{\lambda}$ assume $v = \frac{x^{\theta}R^{\lambda}}{\theta} - q$, therefore $\dot{v} = x^{\theta-1}\dot{x}R^{\lambda} = x^{\theta-1}\dot{x}R^{\lambda} + \frac{\lambda x^{\theta}R^{\lambda-1}\dot{R}}{\theta} - \dot{q}$, hence $\dot{q} = \frac{\lambda x^{\theta}R^{\lambda-1}\dot{R}}{\theta} = \frac{\eta f \lambda x^{\theta+\eta-1}\dot{x}R^{\lambda-1}}{\theta}$.

Here assume $q = \frac{\eta f \lambda x^{\theta+\eta} R^{\lambda-1}}{\theta.\ \overline{\theta+\eta}} - r$, thence $\dot{q} = \frac{\eta f \lambda x^{\theta+\eta-1}\dot{x}R^{\lambda-1}}{\theta} + \frac{\eta f \lambda.\ \overline{\lambda-1}.\ x^{\theta+\eta}R^{\lambda-2}\dot{R}}{\theta.\ \overline{\theta+\eta}} - \dot{r}$, therefore

$\dot{r} = \frac{\eta f. \lambda. \overline{\lambda-1}. x^{\theta+\eta}R^{\lambda-2}\dot{R}}{\theta.\ \overline{\theta+\eta}}$ or $\dot{r} = \frac{\eta^2 f^2 \lambda. \overline{\lambda-1}. x^{\theta+2\eta-1}\dot{x}R^{\lambda-2}}{\theta.\ \overline{\theta+\eta}}$.

Now assume $r = \frac{\eta^2 f^2 \lambda.\ \overline{\lambda-1}.\ x^{\theta+2\eta}R^{\lambda-2}}{\theta.\ \overline{\theta+\eta}.\ \overline{\theta+2\eta}} - s$, thence by the same steps we find $\dot{s} = \frac{\eta^3 f^3 \lambda. \overline{\lambda-1}. \overline{\lambda-3}. x^{\theta+3\eta-1}\dot{x}R^{\lambda-3}}{\theta.\ \overline{\theta+\eta}.\ \overline{\theta+2\eta}.}$,

and so we may assume

$$s = \frac{\eta^3 f^3 \lambda.\ \overline{\lambda-1}.\ \overline{\lambda-2}.\ \overline{\lambda-3}.\ x^{\theta+3\eta}R^{\lambda-3}}{\theta.\ \overline{\theta+\eta}.\ \overline{\theta+2\eta}.} - t,$$

there the value of v may be expressed by this series,

viz. $v = \frac{x^{\theta}R^{\lambda}}{\theta} - \frac{\eta f \lambda x^{\theta+\eta}R^{\lambda-1}}{\theta.\ \overline{\theta+\eta}} + \frac{\eta^2 f^2 \lambda. \overline{\lambda-1}.\ x^{\theta+2\eta}R^{\lambda-2}}{\theta.\ \overline{\theta+\eta}.\ \overline{\theta+2\eta}} -$

$$\frac{\eta^3 f^3 \lambda.\ \overline{\lambda-1}.\ \overline{\lambda-2}\ x^{\theta+3\eta}R^{\lambda-3}}{\theta.\ \overline{\theta+\eta}.\ \overline{\theta+2\eta}.\ \overline{\theta+3\eta}} + \text{\&c.}$$

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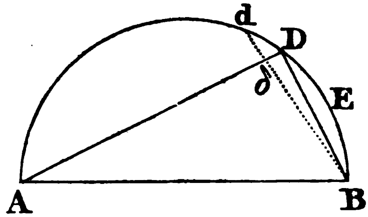
I cannot say but I was vastly pleased with this discovery, as I then thought it, because it not only gives series, more simple than the Newtonian, for the circle and hyperbola, but with a greater degree of convergency, as you may perceive from what I sent you. But I shall give another example or two in the circle.



Let ADB be a semicircle, and BD any arch whose chord is c, and $C=\sqrt{d^2-c^2}$ the chord of its supplement, the diameter being d, then the figure being constructed as above, 'tis manifest the triangles ADB and Dδd are similar; hence $\sqrt{d^2-c^2}:c::\dot{c}:\frac{\dot{c}c}{\sqrt{d^2-c^2}}=\delta$D, therefore $\frac{c^2\dot{c}}{2\sqrt{d^2-c^2}}$ is the area of the elementary triangle δDB or dBD, which is the fluxion of the segment DBED; hence by comparing this with the above ordinate we have $v=\frac{c}{2C}\times\overline{\frac{c^2}{1.3}-\frac{c^4}{3.5.C^2}+\frac{c^6}{5.7.C^4}-\frac{c^8}{7.9.C^6}}$ &c. for the area DBED. Again, let the tangent of any arch of the above circle be t, and its sine s, d being put equal to 2, then for the arch we have $2s^2\times\overline{\frac{1}{2t}+\frac{t}{1.3}-\frac{t^3}{3.5}+\frac{t^5}{5.7}-\frac{t^7}{7.9}}+$ &c., which converges faster than that deduced from the Newtonian series above mentioned, that being $t-\frac{1}{3}t^3+\frac{1}{5}t^5-\frac{1}{7}b^7+$ &c. In the above series putting $t=1$, then $2s^2=1$, and we have $\frac{1}{1.3}-\frac{1}{3.5}+\frac{1}{5.7}-\frac{1}{7.9}+$ &c. $=$ to the arch of 45 degrees, made less by $\frac{1}{2}$, as De Moivre has found from

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other principles. But since I wrote to you last, I have fallen upon a method of transmuting lines into others having any degree of convergency, so that I think from hence the sums of series may be obtained with less trouble than by Mr. Stirling's method.

My method is this. By the fifth prop. of Newton's Quadratures, the area of any binomial curve whose ordinate is $x^{\theta-1} \times \overline{e+fx^{\eta}}|^{\lambda-1}$ (putting $r=\theta+\lambda\eta$) is

$$v=\left(\frac{x^{\theta}}{e\theta}-\frac{frx^{\theta+\eta}}{e^{2}.\theta.\overline{\theta+\eta}}-\frac{f^{2}r.\overline{r+\eta}.x^{\theta+2\eta}}{e^{3}\theta.\overline{\theta+\eta}.\overline{\theta+2\eta}}-\right.$$

$$\left.\frac{f^{3}.r.\overline{r+\eta}.\overline{r+2\eta}.x^{\theta+3\eta}}{e^{4}.\theta.\overline{\theta+\eta}.\overline{\theta+2\eta}.\overline{\theta+3\eta}}+\&c.\right) \times \overline{e+fx^{\eta}}|^{\lambda}.$$ Let both sides of the equation be multiplied by $\frac{x^{\eta-1}\dot{x}}{\overline{e+fx^{\eta}}|^{\lambda}}$, then we [have] $vx^{\eta-1}\dot{x} \times \overline{e+fx^{\eta}}|^{-\lambda} = \frac{x^{\theta+\eta-1}\dot{x}}{e\theta}-\frac{frx^{\theta+2\eta-1}\dot{x}}{e^{2}.\theta.\overline{\theta+\eta}}+$

$\frac{f^{2}.r.\overline{r+\eta}.x^{\theta+3\eta-1}\dot{x}}{e^{3}.\theta.\overline{\theta+\eta}.\overline{\theta+2\eta}}-$ &c. The fluent of the latter part of the equation may be found from the common methods; and for the former let us assume F, $vx^{\eta-1}\dot{x}.\overline{e+fx^{\eta}}|^{-\lambda}=av \times \overline{e+fx^{\eta}}|^{1-\lambda}-q$, hence we have $av.\overline{1-\lambda}.\eta fx^{\eta-1}\dot{x} \times \overline{e+fx^{\eta}}|^{-\lambda}+a\dot{v} \times \overline{e+fx^{\eta}}|^{1-\lambda}-\dot{q}=vx^{\eta-1}\dot{x} \times \overline{e+fx^{\eta}}|^{-\lambda}$, therefore putting $a=\frac{1}{\eta f.\overline{1-\lambda}}$, then $q=\mathrm{F}, a\dot{v}.\overline{e+fx^{\eta}}|^{1-\lambda}=\mathrm{F}, ax^{\theta-1}\dot{x}. \times \overline{e+fx^{\eta}}|^{0}=\frac{ax^{\theta}}{\theta}$;

consequently $v=\frac{x^{\theta}}{\eta.f.\overline{1-\lambda}}+\frac{x^{\theta+\eta}}{e.\theta.\overline{\theta+\eta}}-\frac{f.r.x^{\theta+2\eta}}{e^{2}.\theta.\overline{\theta+\eta}.\overline{\theta+2\eta}}+$

$\frac{f^{2}r.\overline{r+1}.x^{\theta+3\eta}}{e^{3}.\theta.\overline{\theta+\eta}.\overline{\theta+2\eta}.\overline{\theta+3\eta}}-$&c. $\times \eta f.\overline{1-\lambda}.\overline{e+fx^{\eta}}|^{\lambda-1}$,

which manifestly converges faster than before. If

both sides of this equation are multiplied by $x^{\theta-1}\dot{x} \times \overline{e+f.x^{\eta}}\big|^{1-\lambda}$ / $\eta f.\overline{1-\lambda}$, we shall have $\frac{vx^{\eta-1}\dot{x}\times\overline{e+fx^{\eta}}\big|^{1-\lambda}}{\eta f.\overline{1-\lambda}} =$

$$\frac{x^{\theta+\eta-1}\dot{x}}{\eta f\overline{1-\lambda}} + \frac{x^{\theta+2\eta-1}\dot{x}}{e\theta.\overline{\theta+\eta}} - \frac{frx^{\theta+3\eta-1}\dot{x}}{e^2\theta.\overline{\theta+\eta}.\overline{\theta+2\eta}} +$$

$\frac{f^2r.\overline{r+\eta}.x^{\theta+4\eta-1}\dot{x}}{e^3\theta.\overline{\theta+\eta}.\overline{\theta+2\eta}.\overline{\theta+3\eta}}$ — &c.; for the fluent of the former part we may assume $\acute{a}v.\overline{e+fx^{\eta}}\big|^{2-\lambda}-r$, and from hence $\frac{vx^{\eta-1}\dot{x}\times\overline{e+fx^{\eta}}\big|^{1-\lambda}}{\eta f.\overline{1-\lambda}} = \acute{a}.\overline{2-\lambda}.\eta fx^{\eta-1}\dot{x}\times$ $\overline{e+fx^{\eta}}\big|^{1-\lambda}+\acute{a}\dot{v}\times\overline{e+fx^{\eta}}\big|^{2-\lambda}-\dot{r}$, where making $\acute{a}=$ $\frac{1}{\eta^2f^2.\overline{1-\lambda}.\overline{2-\lambda}}$, then $\dot{r} = \acute{a}\dot{v}\times\overline{e+fx^{\eta}}\big|^{2-\lambda} =$ $\acute{a}x^{\theta-1}\dot{x}\times\overline{e+fx^{\eta}}\big|^{1}$, and therefore $r = \frac{\acute{a}ex^{\theta}}{\theta} + \frac{\acute{a}fx^{\theta+\eta}}{\theta+\eta}$, from whence we deduce $v = \overline{\frac{ex^{\theta}}{\eta^2f^2.\overline{1-\lambda}.\overline{2-\lambda}.\theta} +}$

$$\overline{\frac{\eta.\overline{2-\lambda}+1}{\eta^2f.\overline{1-\lambda}.\overline{2-\lambda}.\overline{\theta+\eta}}x^{\theta+\eta} + \frac{x^{\theta+2\eta}}{e\theta.\overline{\theta+\eta}.\overline{\theta+2\eta}} -}$$

$\overline{\frac{frx^{\theta+3\eta}}{e^2\theta.\overline{\theta+\eta}.\overline{\theta+2\eta}.\overline{\theta+3\eta}}+\&c.}\times\eta^2f^2.\overline{1-\lambda}.\overline{2-\lambda}.\overline{e+fx^{\eta}}\big|^{\lambda-2}$, which converges faster than the former value of v; and thus we may proceed till the last or far distant terms may be neglected. But the method of continuing this to infinity may be deduced after this manner. Let the value of v be supposed expressed by a series of this form, viz. $v=\overline{Ax^{\theta}+Bx^{\theta+\eta}+Cx^{\theta+2\eta}+Dx^{\theta+3\eta}+Ex^{\theta+4\eta}+\&c.}\times Q\times\overline{e+fx^{\eta}}\big|^{\lambda-\omega}$, where ω represents some affirmative whole number, then both sides of the equation being mul-

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tiplied by $\frac{\overline{e+fx^{\eta}}\big|^{\omega-\lambda}.x^{\eta-1}\dot{x}}{Q}$, we have

$$\frac{vx^{\eta-1}\dot{x}\times\overline{e+fx^{\eta}}\big|^{\omega-\lambda}}{Q}=Ax^{\theta+\eta-1}\dot{x}+Bx^{\theta+2\eta-1}\dot{x}+$$

$Cx^{\theta+3\eta-1}\dot{x}+Dx^{\theta+4\eta-1}\dot{x}+$ &c. For the fluent of the first part $vx^{\eta-1}\dot{x}\times\overline{e+fx^{\eta}}\big|^{\omega-\lambda}$ assume $av\times\overline{e+fx^{\eta}}\big|^{\omega-\lambda+1}-p$, then $vx^{\eta-1}\dot{x}\times\overline{e+fx^{\eta}}\big|^{\omega-\lambda}=\overline{\omega-\lambda+1}\,av\eta fx^{\eta-1}\dot{x}\times\overline{e+fx^{\eta}}\big|^{\omega-\lambda}+a\dot{v}\times\overline{e+fx^{\eta}}\big|^{\omega-\lambda+1}-\dot{p}$, where putting $a=\frac{1}{\eta f\,\overline{\omega-\lambda+1}\times Q}$ we have $\dot{p}=a\dot{v}\times\overline{e+fx}\big|^{\omega-\lambda+1}$ or $\dot{p}=ax^{\theta-1}\dot{x}\times\overline{e+fx^{\eta}}\big|^{\omega}$, and thence

$$p=\frac{ae^{\omega}x^{\theta}}{\theta}+\frac{a\omega fe^{\omega-1}x^{\theta+\eta}}{\theta+\eta}+\frac{a\omega.\overline{\omega-1}.f^{2}e^{\omega-2}x^{\theta+2\eta}}{1.2.\overline{\theta+2\eta}}+\text{ \&c.,}$$

and from thence

$$v=\left(\left.\begin{array}{c}\frac{ae^{\omega}x^{\theta}}{\theta}+a\omega fe^{\omega-1}\\ +\quad A\end{array}\right\}x^{\theta+\eta}\begin{array}{c}+\\ \\ +\end{array}\left.\begin{array}{c}\frac{a\omega.\overline{\omega-1}.f^{2}.e^{\omega-2}}{1.\overline{\theta+2\eta}}\\ \frac{B}{\theta+2\eta}\end{array}\right\}x^{\theta+2\eta}\begin{array}{c}+\\ \\ +\end{array}\left.\begin{array}{c}\frac{\omega.\overline{\omega-1}.\overline{\omega-2}.f^{3}.e^{\omega-3}}{1.2.3.\overline{\theta+3\eta}}\\ \frac{C}{\theta+3\eta}\end{array}\right\}x^{\theta+3\eta}+\text{ \&c.}\right)\times\frac{\overline{e+fx^{\eta}}\big|^{\lambda-\omega-1}}{a}$$

and from thence the fifth value of $v=$

$$\left(\frac{e^{3}x^{\theta}}{\theta\eta^{4}f^{4}.\overline{1-\lambda}.\overline{2-\lambda}.\overline{3-\lambda}.\overline{4-\lambda}}+\right.$$

$$\frac{e^{2}.\eta.\overline{\overline{4-\lambda}+3\theta}.x^{\theta+\eta}}{\eta^{4}f^{3}.\overline{1-\lambda}.\overline{2-\lambda}.\overline{3-\lambda}.\overline{4-\lambda}.\theta.\overline{\theta+\eta}}+$$

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$$\frac{e\eta.\ \overline{4-\lambda}.\ \overline{2\theta+\eta}.\ \overline{3-\lambda+3\theta}.\ \overline{\theta+\eta}.\ x^{\theta+2\eta}}{\eta^4 f^2.\ \overline{1-\lambda}.\ \overline{2-\lambda}.\ \overline{3-\lambda}.\ \overline{4-\lambda}.\ \theta.\ \overline{\theta+\eta}.\ \overline{\theta+2\eta}}+$$

$$\frac{\theta.\ \overline{\theta+\eta}.\ \overline{\theta+2\eta+\eta}.\ \overline{4-\lambda}.\ \eta.\ \overline{3-\lambda}.\ \overline{\theta+\eta}.\ \overline{2-\lambda}.}{\eta^4 f.\ \overline{1-\lambda}.\ \overline{2-\lambda}.\ \overline{3-\lambda}.\ \overline{4-\lambda}.\ \theta.\ \overline{\theta+\eta}.\ \overline{\theta+2\eta}.\ \overline{\theta+3\eta}}.\ x^{\theta+3\eta}$$

$$\frac{x^{\theta+4\eta}}{e\,\theta.\ \overline{\theta+\eta}.\ \overline{\theta+2\eta}.\ \overline{\theta+3\eta}.\ \overline{\theta+4\eta}}-$$

$$\left.\frac{frx^{\theta+5\eta}}{e^2\theta.\ \overline{\theta+\eta}.\ \overline{\theta+2\eta}.\ \overline{\theta+3\eta}.\ \overline{\theta+4\eta}.\ \overline{\theta+5\eta}}+\&c.\right)\times$$

$\eta^4 f^4.\ \overline{1-\lambda}.\ \overline{2-\lambda}.\ \overline{3-\lambda}.\ \overline{4-\lambda}.\ \overline{e+fx^\eta}\big|^{\lambda-\eta}$. Now if $\frac{\dot{x}}{1+x^2}=\dot{v}$, then here

$$v=\left(\frac{x}{4^3.\ 2.\ 3}+\frac{11\,x^3}{4^3.\ 2.\ 3.\ 1.\ 3}+\frac{57\,x^5}{4^3.\ 2.\ 3.\ 1.\ 3.\ 5.}+\frac{255\,x^7}{4^3.\ 2.\ 3.\ 1.\ 3.\ 5.\ 7}+\frac{x^9}{1.\ 3.\ 5.\ 7.\ 9}-\frac{x^{11}}{3.\ 5.\ 7.\ 9.\ 11}+\frac{x^{13}}{5.\ 7.\ 9.\ 11.\ 13}-\frac{x^{15}}{7.\ 9.\ 11.\ 13.\ 15}+\&c.\right)\times\frac{4^3.\ 2.\ 3}{\overline{1+x^2}\big|^4},$$

and this must converge prodigiously swifter than what it would do before it was transmuted.

I designed to have sent you also the fluent $\frac{\dot{x}}{1+x}$ transmuted, and an example in numbers to compare with page 27 of Mr. Stirling's book De Summatione &c., but the carrier (by whom this comes) waits, so am obliged to finish. If you think this merits a place in the Acts of your Society, I will immediately prepare a paper for that purpose, which will be a means to make me acquainted with them, and with less difficulty to be admitted a Member [k], when you shall think fit to propose me, knowing that your interest is sufficient. I am the more desirous of being chosen at this

[k] The name of George Anderson does not appear in the list of those who have been admitted as Fellows of the Royal Society.

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time, because it would fix me more in the esteem of some people, whose good opinion may be of great service to me.

Having lately been looking into Mr. Stirling's book, I find the very series I sent you for binomial curves; it is in page 114, deduced from the method of differences. This letter will be delivered by our porter; therefore, if you have Mr. Machin's paper, I shall be glad to see it, and a line when you are at leisure, with your opinion of my method for approaching the sums of series.

I am, with great respect,
your most devoted
and obedient servant,
G. ANDERSON.

Newbottle,
Sept. 17, 1739.

CXXIV.

MACHIN TO ——.

Dear Sir,

I thought I saw, here and there, something very clever in those rules you shewed me yesterday; but now I have narrowly inspected them, I do not wonder at my liking them so much as I did. For indeed I find them all to be nothing else but my own dear offspring, though so barbarously hacked and mangled, in many places, by some clumsy cruel hand, that it was a long time before I was able to know them again.

I must therefore apply to your worship, as a justice of the peace, to take this kidnapper of an old woman,

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the next time you catch her, under your examination, for thus abusing my poor infants and stripping them of their new clothes, and then impudently sending them in such tatterly rags a begging to your worship.

I think I heartily love ingenuity in any one, wherever I can see it, and believe I should be among the first of those who would allow due applause to any ingenious performance, although it were to interfere with my own interest. But there is a sort of false genii that always provokes one's spleen, who, like little children, blinding themselves with their own ignorance, think nobody knows what they do. These know all things only in a superficial manner, and thinking others do the same, are so weak to imagine they can make a different dress pass upon people for a different body. And this, if I am not much mistaken, is the very case with your correspondent. However, I will submit the whole affair to your judgment, and stand decided by it.

The substance of his method is nothing more than the rule which I have given, at the bottom of the 223d page [a], for the correction of the angle B, viz. $\frac{t}{x} \times \overline{M-\mu}$, only so far as it is adapted to the motion from the aphelion instead of the perihelion, in which case μ is $= B + \frac{fR}{H}y$, consequently $\frac{t}{x} \times \overline{M-\mu}$ is $= \frac{t}{x} \times \overline{M - B - \frac{fR}{H}y}$, which is his very rule for the correction, $\frac{b}{a} = \frac{1}{\frac{1}{f} + c} \times \overline{\frac{R\pi}{f} - s}$, as will appear upon

[a] Phil. Trans. Vol. XL. in Machin's paper on Kepler's Problem.

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putting 1 for t, c for z, s for y, RA for M, πA for B, and $R\pi$ for $M-B$.

If he can have any merit at all in this rule, it can be only so far as he pretends to assign some reason for it, all which he is heartily welcome to.

But, because this is nothing but a rule for a further correction ad libitum, and it was necessary for him to have some rule for a first assumption, as seeing that without it the other would be of no use, and yet being unwilling to make that distinction as I have done; this has put the gentleman to some little trouble, and made him guilty of that irregular division of his method, into two rules not opposite, which I took notice of upon first sight of his paper. For, no doubt, he observed that it would require an expense of more art and skill than he was willing to bestow, to disguise my general rule for the first assumption, as he had done the other: and therefore he has recourse to a shorter method. He observes that I have a little private rule, in a corner, at the bottom of page 221, which I make no use of; but it unluckily happens, that it is not general, and what is worse, is so entirely all of one piece, that there is no way of disguising it by putting it into any other dress that can fit it: wherefore to remedy these inconveniences, as to the last, he resolves at once, since he cannot give it any new uncouth form, to cut it, slash it, and deface it, in such a manner as nobody may know it, and yet so as not totally to destroy its use. And so instead of putting, as I have done, $\frac{t}{na} \times \frac{M}{R}$ for the sine of $\frac{1}{n}A$, (or submultiple part of the anomaly of the eccentric,) he puts it for the same part of the arch itself, viz. $\frac{1}{n}A$; and then consequently $\frac{t}{a} \times \frac{M}{R}$ will be equal to A, that

is, $\frac{\text{CA}}{\text{SA}} \times RA = \pi A$, according to his expression, which is his first rule.

But, as I was saying, this rule not being general, to supply the defect he has a very clumsy addition of his own, that may serve to eke out the rule to the middle distances of the orbit; and, as to the parts beyond, towards the perihelion, in comets, he leaves them to shift for themselves, however, wisely taking care to give no improper examples, that may serve to discover the defect. He observes, as any one may, (and which is a corollary to the rule for a further correction, but no part of any rule for a first assumption,) that about the middle distances, the arch RN is nearly equal to SC, and so he substitutes this as a subsidiary rule to help the other out, as if the same quantity was constantly to be used for ever after, throughout the rest of the orbit, although the quantity correspondent to it in the other part perpetually varied, till that took place: and this is the only rule, if it can be called so, which has any thing new, in the whole method.

Now this being the true state of the case, I leave you to judge, whether such a correspondency, in all the rules that are right, could probably have happened, if what he says be true, that he never saw my paper. However, had he understood as well as seen my propositions, he would have known, what neither you nor I have occasion to inform him, that the method he proposes is of itself insufficient, and had it been perfect, it does not seem in what is right to be his own.

Thus inter nos I remain,

Dear Sir,

your much obliged friend

and humble servant,

J. Machin.

Gresham College, Sept. 30, 1739.

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CXXV.

Part of a Letter from MAUPERTUIS to FOLKES, dated at Paris, 12 Feb. 1740, N.S.

J'aurois grand honte, mon cher Monsieur, d'avoir été si longtems sans vous écrire, si des occupations continuelles et indispensables ne m'en avoient empêché. La principale de ces occupations a été la mesure du degré du méridien de Paris à Amiens, et je crois que vous aimerez mieux le détail de cette opération, qu'une plus longue excuse.

Comme nous n'avions aucune raison de douter de l'exactitude de la mesure terrestre de M. Picard, ni plus d'adresse, ni de meilleurs instrumens, que lui pour refaire ses triangles, nous nous en sommes tenus à sa mesure de la distance de Malvoisine à Amiens, de laquelle nous avons pris la partie interceptée entre deux églises cathédrales de Paris et d'Amiens, qui sont si éxactement placées sous un même méridien, qu'on croiroit qu'on auroit destiné ces deux monumens, les plus grands et les plus durables que nous ayons en France, à être les termes de notre mesure. L'arc intercepté entre ces deux églises est de 59530 toises, comme on peut en déduire des opérations de M. Picard, et les points qui terminent cet arc sont le clocher de Notre Dame d'Amiens, et la tour méridionale de Notre Dame de Paris. C'est de cet arc que nous nous sommes proposés de déterminer l'amplitude avec notre secteur, qui est le même que nous avions en Laponie, que nous devons à M. Graham, et qui est si supérieur au secteur de M. Picard.

Nous avons déterminé l'amplitude de cet arc par un grand nombre d'observations de deux étoiles, *α* [de]

Persée, et γ du Dragon. Les amplitudes données par ces deux étoiles, et corrigées par l'aberration, ne différoient pas d'une seule seconde, et etoient de 1°. 2'. 28".

Comparant cette amplitude à la longueur de l'arc, nous trouvons le degré du méridien entre ces deux églises de 57183 T., qui diffère de celui, que nous avons mesuré en cercle polaire, de 255 T. J'ai lu le détail de toute cette opération à l'académie, où M. Cassini n'a pas osé dire un mot, ce qui doit être fort facheux pour lui.

Car vous verrez mieux que moi, Monsieur, par cette opération, 1°, que nous trouvons ce degré plus grand de 200 T. que M. Cassini ne l'avoit trouvé, et plus grand de 123 que M. Picard.

2°. Qu'après tout ce que le gouvernement avoit fait en France, et tant d'opérations de Cassini, on n'avoit pas la longueur du degré si juste que votre Norwood, qui ne paroît s'être servi que de moyens fort grossiers, l'avoit trouvée.

3°. Qu'enfin la différence de 255 T. entre le degré de Paris et celui de Torneå, diffère si peu de celle qui résulte de la théorie de M. Newton, qu'à moins qu'on ne trouve des instrumens bien différens de tous ceux qu'on a, il est impossible que les mesures actuelles s'accordent d'avantage avec cette théorie, si ce n'est par hasard. La comparaison de nos deux degrés de Paris et de Torneå, donne le rapport de l'axe de la terre au diamètre de l'équateur comme 177 à 178.

Si vous jugez à-propos de faire part de cette opération à la Société Royale, je vous en serai fort obligé.

Après cela, Monsieur, vous serez bien surpris quand vous lirez le livre dont je vous envoye quatre exemplaires, dont je vous prie de donner trois à MM. Jurin, Bradley, et Desaguliers que ce livre regarde personnellement. Quoique daté de 1738, il ne paroît que depuis

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peu de semaines à Paris, où il fait grand bruit. M. Desaguliers n'y est pas ménagé, et le pis que je trouve, c'est qu'il ne paroît pas qu'il lui soit facile de répondre. Je serai bien aise de savoir votre sentiment et celui de ces Messieurs sur ce livre, qui achève de faire croire la terre allongée, à ceux qui en doutoient; et par des raisonnemens aux quels il n'y a guères à faire qu'une réponse qui n'est pas honnête.

M. l'abbé Hubert, qui vous rendra cette lettre, a bien voulu se charger de deux livres que je prends la liberté de vous prier d'accepter, non que je croye qu'ils contiennent rien que vous ne puissiez trouver sur le champ si vous le vouliez, mais parceque vous y trouverez les dessins de toutes les machines hydrauliques que nous connoissons en France.

L'amitié dont vous m'honorez me fait croire que vous prendrez part à ce qui vient de m'arriver. Je viens d'être chargé de travailler à perfectioner la Navigation avec un brevet de mille écus de pension. Cette place qu'on a créée pour moi m'est si honorable que je ne crains que de ne pouvoir pas la bien remplir, car les fonctions en sont fort étendues. Le résultat de ce travail doit être un livre classique qui s'enseignera, par ordre du roi, dans toutes les écoles d'hydrographie, d'où il banira un grand nombre de mauvais livres qui s'y enseignent actuellement. Car nous n'avons pas un livre bien fait sur cette matière si importante.

Vous me faites grand plaisir,

Monsieur, &c.

This extract from Maupertuis' letter is copied by Machin. He was at the time secretary of the Royal Society, to which the intelligence was desired to be communicated. The book, of which Maupertuis sends four copies to Folkes, was, most pro-

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bably, "Examen des trois dissertations que M. Desaguliers a "publiées sur la figure de la terre, inserées dans les Transac- "tions Philosophiques de la Societé Royale de Londres, Nos. "386, 387, et 388. Oldenbourg, 1738, 12°." Lalande mentions it in his Bibliographie Astronomique (p. 407), and from what he says on two preceding articles, it should seem not unlikely that the work was Maupertuis' own writing.

CXXVI.

G. ANDERSON TO JONES.

Sir, Leyden, Sept. $\frac{16}{27}$, 1740.

Being now somewhat settled, and got into the train of studies and exercises we are to pursue this year, I make use of this first occasion to pay my respects to you, and at the same time to send you the theorems, I mentioned, for finding the logarithm from the number given, or the contrary, which are but corollaries to a theorem I sent you, some time past, from Newbottle; and I am inclined to think, would my time allow me to reexamine another theorem, I also sent you from the same place, for squaring binomial curves, others might thence be deduced, which would still do the business with less trouble. But to come to the theorems I am speaking of; let $a-x$, a, $a+x$ be any three numbers in arithmetic proportion, whose logarithms I denote by $L_{\prime}$, L, L', respectively, then putting $n = 0,4342944819$ &c., to find L', from a being given, we have the three following theorems, and putting $L' - L_{\prime} = l$, the value of x is had by the last three theorems.

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$$\left.\begin{array}{l}
1.\quad L' = L + \dfrac{2nx}{2a+x} \\[2ex]
2.\quad L' = L + \dfrac{3n \times \overline{2a+x}}{6a+x+\dfrac{6a^2}{x}} \\[3ex]
3.\quad L' = L + \dfrac{n}{3}\,\dfrac{15 \times \overline{2a+x}|^2 - 4x^2}{\overline{2a+x} \times \overline{10a+x+\dfrac{10a^2}{x}}}
\end{array}\right\}\text{ too little by}$$

$$\left.\begin{array}{ll}
1. & \dfrac{2nx^3}{3 \times \overline{2a+x}|^3}\ \&c. \\[2ex]
2. & \dfrac{4nx^5}{45 \times \overline{2a+x}|^5}\ \&c. \\[2ex]
3. & \dfrac{4nx^7}{175 \times \overline{2a+x}|^7}\ \&c.
\end{array}\right\}\begin{array}{c}\text{and}\\ \text{on}\\ \text{the}\\ \text{con-}\\ \text{trary}\end{array}\left\{\begin{array}{l}
x = \dfrac{2la}{2n-l} \\[2ex]
x = \dfrac{6la}{3.\overline{2n-l} + \dfrac{2l^2}{n}} \\[3ex]
x = \dfrac{\overline{30n^2+8l^2} \times la}{15n^2 \times \overline{2n-l} + 4l^2 \times \overline{12n-l}}
\end{array}\right.$$

Where I must observe the lower theorem both for L' and x is always nearer the truth than that immediately above, and consequently the lowest in each is the nearest of all. I have also reduced each of them to that form which will give the least trouble in practice; for in the second and third for finding L', the divisor of $6a^2$ and $10a^2$ (viz. x) is commonly unity, because we may commonly suppose the logarithm of the number, immediately preceding that required, to be given.

The second in each is sufficient for practice, since thereby we can commonly have the logarithm or number true to ten or fifteen places. For example, let $a = 100$, $x = 1$, then $L = 2{,}000000$, &c., whence by the second theorem $L' = 2 + \dfrac{3n \times 201}{600 + 1 + 60000} = 2.004321373782 4$,

erring but 2 in the last place; the error being somewhat more than $\frac{4n}{45 \times \overline{201}|^{5}}$, and by the first term of the errors in each, we may always know how near we are to [the] truth. I have not set down those for the values of x, but they may easily be had by reducing their values to a series, and comparing it with $a \times \left(-1 + \frac{l}{n} - \frac{l^3}{3n^3} + \frac{2l^5}{45n^5} - \frac{17l^7}{315n^7} + \frac{62l^9}{2835n^9} - \&c.\right)$, which is equal to x.

But if $L_{\prime}$, L' the logarithms of $a-x$ and $a+x$ be given, then L the logarithm of a is had by the three theorems following, each of which is prodigiously near the truth, as appears by the first term of the error annexed to each, viz.

$$\left.\begin{aligned}
L &= \frac{L_{\prime} + L'}{2} + \frac{nx^2}{2a^2 - x^2} \\
L &= \frac{L_{\prime} + L'}{2} + \frac{3nx^2 \times \overline{2a^2 - x^2}}{3 \times \overline{2a^2 - x^2}|^2 - x^4} \\
L &= \frac{L_{\prime} + L'}{2} + \frac{15 \times \overline{2a^2 - x^2}|^2 - 4x^4}{5 \times \overline{2a^2 - x^2}|^2 - 3x^4} \times \\
&\qquad \frac{nx^2}{3 \times \overline{2a^2 - x^2}}
\end{aligned}\right\} \text{too little by}$$

1. $\frac{nx^6}{3 \times \overline{2a^2 - x^2}|^3}$ &c.

2. $\frac{4nx^{10}}{45 \times \overline{2a^2 - x^2}|^5}$ &c.

3. $\frac{4nx^{14}}{175 \times \overline{2a^2 - x^2}|^7}$ &c.

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As my time is pretty much taken up, I have little to spare for mathematical studies, and therefore have met with nothing of that kind that deserves notice, unless it be a demonstration of the general quadrature of the lunular spaces, which I fell upon by reading what Craig has done thereon, page 35 de Calculo Fluentium, which in my opinion is a very inelegant and imperfect performance; for in order to prove that Ap: AH as $1:\sqrt{2}$ (see his second figure, page 35) he lays down a lemma without proof, alleging, "calculum (utpote prolixum) non addo." Where the difficulty lay, or why he was obliged to use the elementa of each arch in order to shew that $Ap:AH::1:\sqrt{2}$, I cannot imagine, for on R (Pl. 3, fig. 6.) with the distance RK, describe the arch $K\alpha\beta$, cutting RA and RH in the points β, α, respectively, and continue the circumference DHA to R, then 'tis plain from the 20th, 3d of Eucl., the angle AKH is double the angle ARH, whence $\alpha\beta=\frac{AH}{2}$, but $RK:RA::\alpha\beta:Ap::1:\sqrt{2}$, for $AR=RK\sqrt{2}$, hence $1:\sqrt{2}::\alpha\beta=\frac{AH}{2}:Ap$, or $Ap:AH::1:\sqrt{2}$, which is the substance of Craig's lemma and its corollary.

But there is no need of the proportion of the arches Ap, AH, in order to measure the lunular segment AHp; for since the sector $R\alpha\beta$ is to the sector RpA as RK^2 is to RA^2, that is, as 1 to 2, 'tis manifest the sector RpA is double the sector $R\alpha\beta$, but by what was shewn above, (viz. that the angle AKH is double $\beta R\alpha$, therefore,) the sector AKH is double $R\alpha\beta$, or equal to the sector ARp, from which take away the common space Apq, then we have the triangle ARq equal to the triangle HqK more by the lunular segment AHp.

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Again, because Hr and RK are both perpendicular to AD, they are parallel, hence by the 37th of the 1st of Euclid, the triangle HqK is equal to rqR, therefore from the equals HqK + AHp = AqR, take the equals HqK = rqR, and there remains the lunular segment AHp equal to the triangle ArR, the thing to be proved.

This solution it seems was first published in the Acta Erudit. 1687, and looked upon as so curious a performance, that Wallis and Gregory each gave their sentiments of it in our Transactions[b], which I have not seen, but it is natural to suppose Craig must have read their papers, and therefore his solution is not longer than theirs: if so, 'tis manifest they had not sufficiently considered the figure, for the demonstration above does not require the fifth part of that of Craig, and besides nothing is taken for granted but what is shewn from the Elements; and it was this brevity, in a matter that had been considered by so many eminent men, that made me fancy you would not be displeased with what I have sent, being sensible the proposition itself is not of any great consequence.

I have visited most of the booksellers here, which are not a few, but I cannot say they are over and above stocked with mathematical books, though here are some; particularly they have printed Newton's Arithmetic, and all Keill's pieces, each in quarto; to the first is added Colson, Maclaurin, Campbell, and De Moivre's papers, (out of the Transactions,) relating to the roots of equations; and in the latter is contained the Astronomy, Physics, Trigonometry, and the paper about the centripetal force; both neat editions, and corrected by 'sGravesande, who is going to reprint his Physics with great improvements. They have

[b] Vol. XXI. p. 411.

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Plate 3.

Fig. 1. p. 287.

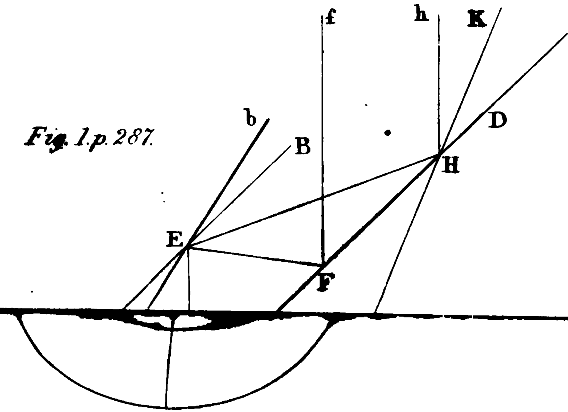
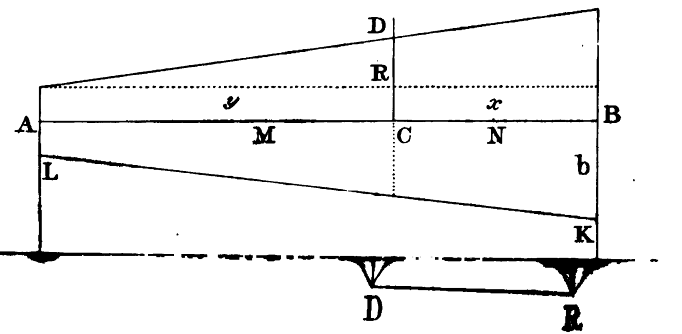


Fig. 3. p. 308.



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here another book printed at Leipsic, by one Hausen a professor there, which I think is very well done. It treats of Arithmetic, Geometry, Trigonometry of both kinds, and the Conic Sections. I presume he has seen your Synopsis, there being something taken thence about making the sines and tangents. The price of the new Comment on the Principia is about ten shillings, but I cannot hear any thing of Cassini's Astronomy, nor the Paris edition of Newton's Fluxions. They have all Hugens' pieces in four vols. quarto, very cheap, and I hear they expect shortly from Germany Bernoulli's Works in four volumes quarto, but I cannot learn which of them it is.

The sales for books are begun: so, sir, if I can be of any service, I beg you'll favour me with your commissions, which I shall execute to the best of my knowledge; I being desirous of any occasion, whereby I may shew you how much

I am

your most obedient and

devoted servant,

GEO. ANDERSON.

P.S. Pray, sir, if you favour me with a letter, please let me know what is the proportion of the force the wedge has, in splitting timber &c., to the force impressed: for I find, from what Rowning has said thereon, the proportions assigned by different writers are not the same (see Fig. 36, Pl. viii. of Rowning's Philos.). He says the force impressed on the wedge is to that wherewith it impels the sides of the body to be separated as $\frac{1}{2}$DB to BC, which Keill makes as 2DB to BC; but to me it appears they are both mistaken; for I think it should be, as the length of the

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arch described on C with the distance CD, and contained by the sides CA, CB, to CD, that is, as twice the arch whose tangent is DB to radius CD.

Please direct for me at Mr. Fournier's, the French Arms, on the Long Bridge, Leyden.

CXXVII.

EARL OF MACCLESFIELD TO JONES.

Dear Sir,

I received yours yesterday in the afternoon, and the shortness of the days, together with my having company at dinner, prevented my writing by the last post, but I have by this post written a long and pressing letter to the Chancellor [c] in behalf of Mr. Bradley, whom you know how rejoiced I should be to have it in my power to serve. But my way of thinking and voting is a great obstruction to having any interest at court, nor can I think of any other way I can recommend Mr. Bradley to this professorship, than by means of the Chancellor and Archbishop [d]. I do not know upon what footing my kinsman Mr. Justice Parker is with Lord Chief Justice Willes, it is certain the latter has great interest with Sir Robert, and it may be of advantage if cousin Parker would state the thing in a proper light to Willes, and represent to him how much it will be for Sir Robert's discredit, that the only man in England fit to succeed Halley at Greenwich, confessed so by all who know any thing of astronomy, should be put by, and the finest instrument in the universe put into the hands of a person unable to make a proper use of it. I dare say my cousin's

[c] Lord Hardwicke. [d] Dr. Potter.

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friendship for me will make him undertake this part, and endeavour to prevail upon Willes to interest himself for Mr. Bradley, if there be no impropriety in the thing: I wish, therefore, you would talk to him from me, and shew him both this and the enclosed copy of my letter to the Chancellor, which will contribute possibly to make him master of the case, and enable him to talk to Willes the better upon it. If your health should not permit you to call upon Mr. Justice Parker yourself, I would have you send for my cousin John, and explain the thing to him, and desire him in my name to go to the Judge, and represent the thing to him, and desire his assistance in the affair, so far as may be consistent with the footing he may at present be upon with Willes, or, in short, so far as he shall think convenient. Mr. Folkes might speak to the Duke of Richmond; you to Sir Charles Wager; and Lord Charles [e] to somebody or other, for I think nothing should be left undone that can at present be done for our friend, this being the only time for advancing him in the way he chooses.

Lady Macclesfield and myself have both had colds, but both are better at present, and hope to arrive in good health in town on Saturday about five o'clock, when, if I do not see you, I should be glad to hear by a line or two what prospect there may be of Mr. Bradley's succeeding.

I am, dear Sir,
your affectionate friend
and humble servant,

MACCLESFIELD.

Shirburn, Jan. 14, 1741-42.

This and the following letter have been printed in the Memoirs prefixed to Bradley's Miscellaneous Works, pp. xlvi. xlvii.

[e] Probably Lord Charles Cavendish.

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CXXVIII.

EARL OF MACCLESFIELD TO LORD HARDWICKE.

My Lord,

Yesterday I received notice that Dr. Halley could not hold out longer than a day or two, and I hope your lordship will pardon my troubling you with this in behalf of my friend Mr. Bradley, whom you formerly seemed inclined to serve whenever Dr. Halley's death should make a vacancy at Greenwich.

It is not the salary annexed to that professorship, which makes me so desirous that Mr. Bradley should succeed Dr. Halley in it, but there is a credit attending such a professorship when possessed by a man of real merit; and it is a disappointment to, and a sort of slight put upon such a person, when upon a vacancy he is neglected, and a person much inferior to him is preferred before him: and give me leave to say, that must be Mr. Bradley's case, whosoever except himself succeed Dr. Halley; and besides Mr. Bradley's abilities, he has so very great a liking to the practical part of astronomy, the making observations, that on that score it would be extremely agreeable to him, and the science would have the greatest reason to expect to receive very considerable improvements from his observations.

But it is not only my friendship for Mr. Bradley that makes me so ardently wish to see him possessed of the professorship; it is my real concern for the honour of the nation with regard to science. For as our credit and reputation have hitherto not been inconsiderable amongst the astronomical part of the world, I should be extremely sorry we should forfeit

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it all at once by bestowing, upon a man of inferior skill and abilities, the most honourable, though not the most lucrative, post in the profession, (a post which has been so well filled by Dr. Halley and his predecessor,) when at the same time we have amongst us a man known, by all the foreign, as well as our own astronomers, not to be inferior to either of them, and one whom Sir Isaac Newton was pleased to call the best astronomer in Europe. This will, I flatter myself, plead my excuse, if I should appear a little importunate in pressing your lordship to intercede early and earnestly in favour of Mr. Bradley, nor can I apply on this occasion to a more proper person than [your lordship]. For as this place has no relation to any department of the administration, but its sole business and view are the advancement and improvement of the science, that is of use to mankind in general, but more particularly so to us, as a trading nation and the chief of the maritime powers; this, I say, being the nature of the place, to whom can the recommendation to it more properly belong than to your lordship, who, not only in private character, but by your public office likewise, are the patron of learning and learned men in general; it was upon this foot that my father, when in the post which you now enjoy, took upon him to recommend Dr. Halley to the royal professorship at Greenwich, and Mr. Bradley to the Savilian at Oxford, and succeeded in both his recommendations; and he always thought it for his honour to have recommended two so able men. And I dare assure your lordship, that, if you shall be pleased to espouse Mr. Bradley's interest, you will have the satisfaction to find your recommendation of him approved and applauded universally by those, who are versed in those studies, both at home and abroad. As Mr. Bradley's abilities

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in astronomical learning are allowed and confessed by all, so his character in every respect is so well established and so unblemished, that I defy the worst of his enemies (if so good and worthy a man have any) to make even the lowest or most trifling objection to it. After all, it may be said, if Mr. Bradley's skill is so universally acknowledged, and his character so established, there is little danger of opposition, since no competitor can entertain the least hope of success against him. But, my lord, we live in an age when most men, how little soever their merit may be, seem to think themselves fit for whatever they can get, and often meet with some people, who by their recommendations of them appear to entertain the same opinion of them; and it is for this reason that I am so pressing with your lordship not to lose any time, as I am confident you would be sorry the professorship should be given to a person unqualified for it, and the finest instrument, perhaps in the universe, put into the hands of a person unable to make a proper use of it, and this to the prejudice of the best qualified and most able astronomer, that not only this nation, but probably all the world can at present shew.

But I forgot how precious your lordship's time is, &c.

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CXXIX.

EARL OF MACCLESFIELD TO JONES.

Dear Sir,

The reason of my not having sooner acknowledged the receipt of several letters, I have lately received from you, was the desire I had to send a full answer to the several particulars mentioned therein, and to give Mr. Machin satisfaction in the several points, in which he desired it. But the weather has been so unfavourable of late for astronomical observations, that I am sorry I cannot do it in a more perfect manner. And yet, as I shall in a day or two turn the quadrant to the north, I must for the present content myself with sending him only an observation or two of the two stars he mentioned, and a very few of the sun.

With regard to the barometer and thermometer, I can give a more satisfactory account, and have enclosed an extract from my book of observations, which will shew him the height of the quicksilver in the barometer, and two of Bewly's thermometers, one constructed according to De L'Isle's, the other to Fahrenheit's scale. This extract contains the observations of the state of the atmosphere for the month of December 1741, and part of January 1742; it then gives the observations made from the first of June till after the autumnal equinox, and to make it the more perfect I have added, at the end, a few made about the vernal equinox. The time mentioned in this extract is that given by my two clocks, which were set to go sidereal

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time, but, not having been at all altered in more than three quarters of a year, may well be supposed not to shew true sidereal time; but as on many days I have, by prefixing the algebraic affirmative mark, distinguished that time to be the noon of that day, it is very easy to reduce the other times to the proper hour of the common day. And this (by the by) may be of use to remove the difficulty Mr. Machin seemed to complain of in the former observations, with respect to determining the beginning or ending of the natural day. In some places notice is taken of the rain and wind, but as that happens but rarely, it will be rightly concluded that those things were only entered in the book occasionally, and not for a constancy. However, when I found it mentioned in the book, I put it down in the extract.

I heard some time since, from Dr. Bradley, that his observations of the sun, at the last equinox, did not correspond at all with those made by me, supposing the latitude of Greenwich to be (according to Mr. Flamsteed) 51° 28′ 30″, but that in order to make them correspond, the latitude of Greenwich must be 20″ more, or 51° 28′ 50″. In order to find out the occasion of this difference, Dr. Bradley desired me to observe the meridian altitudes of thirteen stars of very different declinations; and upon a comparison of the observations made by me three several nights, with those made by him the same nights, ten of the thirteen made the latitude of Greenwich 51° 28′ 50″, and the remaining three still greater: so that, till the latitudes of both places are exactly determined, I think there is no reason to hope our observations will agree. I own I think myself pretty certain of the exactness of mine, which, however, I am determined to examine accurately when the instrument is turned. Not that I

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would impeach the accuracy of Mr. Flamsteed's observatious; but it is possible he might be mistaken in adding to his observed zenith distance 15″, which he calls error divisionum, and for which it is possible there might be no real foundation, but that he might be induced to believe his instrument not to be a true quadrant, merely from finding his observed altitudes of the stars not to correspond with the table of refractions he had composed. However that be, if in making use of his observations of the pole star, that allowance of 15″ be omitted both above and below the pole, and the other numbers be made use of as given by him, the latitude of Greenwich will be found as follows:

	°	′	″
Least zenith distance of pole star	36	8	0
Error of instrument .... +		1	10
Refraction .... +			36
Least zenith distance correct......	36	9	46
Greatest correct	40	52	36
Difference	4	42	50
½ Difference...........................	2	21	25
Least distance correct...............	36	9	46
Complement of latitude	38	31	11
Latitude................................	51	28	49
Greatest	40	50	45
Error of instrument .... +		1	10
Refraction .... +			41
Greatest zenith distance correct...	40	52	36
½ Difference........................... −	2	21	25
Complement of latitude............	38	31	11
Latitude................................	51	28	49

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Which latitude is extremely near to, or may be looked upon as the same with, what results from the comparison of the observations made by Dr. Bradley and myself. And as Mr. Flamsteed gives no reason for making that allowance for the error divisionum, as he terms it, I may be the less blamable for supposing it possible for him to be mistaken in it. But when Dr. Bradley is returned to Greenwich, and I have turned my quadrant again to the south, for observing the solstice, I hope we shall, by comparing repeated observations, be able at last to clear up and settle this affair.

You must have heard, when in the country, of the dreadful fire that happened at Brightwell, by which numbers of poor people were ruined, and amongst the poor sufferers was one who kept a public house, which was entirely consumed, and in it a great quantity of liquor which had paid excise; the particulars whereof are in an enclosed paper. I should think it but justice, in the Commissioners of the Excise, to refund to him, on this occasion, the duty for so much of his stock as he can prove was consumed by the fire; and I should be obliged to you, if you would recommend the affair to Mr. Eyres, to whom you may make use of my name, and, with my service to him, express my desire that he would be so good to obtain this favour for this unfortunate man, if such things can be done consistently with the rules of the board.

I this day received a letter from Mr. Sutton relating to Kyte the painter, which I enclose to you, and should be glad to know whether you have any further directions for Mr. Sutton in that affair, which I will most readily transmit to him.

I received, by the carrier, the book of observations,

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and, by the post, the printed paper, for both which I return you my thanks.

All here join in service to you and Mrs. Jones.

I am, dear Sir,

your very affectionate and faithful friend,

MACCLESFIELD.

Shirburn, Thursday Night,
Oct. 28, 1742.

END OF VOL. I.

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